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THE CALVIN LAB: BIO-ORGANIC CHEMISTRY GROUP AT THE
UNIVERSITY OF CALIFORNIA, BERKELEY, 1945-1963

Volume II

Interviews conducted by Vivian and Sheila Moses
December 1995 - September 1997

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Chapter 24

SAM ARONOFF **(with Edith Aronoff)**

Camano Island, Washington

July 8th, 1996

VM = Vivian Moses; SA = Sam Aronoff; EA = Edith Aronoff; SM = Sheila Moses

VM: This is a conversation with Sam Aronoff — and Edith Aronoff?...

SA: Yes.

VM: ...on July 8th, 1996 on Camano Island in Washington.

You, Sam, I gather, were in Berkeley before the war, from the mid-thirties, and so you can remember what the scene was like into which the Calvin activity later developed.

SA: Yes, indeed. It started with Sam Ruben, you know, and Sam Ruben was the first to receive carbon-14 and to realise its potential for tracing carbon metabolism in plants. He and Kamen (I'm not sure of Martin Kamen's precise role in the undertaking but Sam was the chemist, Martin was a physicist)...As you are probably aware, Sam was carrying a thermos bottle one day which was apparently scratched. The bottle had liquid phosgene in it and it broke, scattering the gas throughout the building in which he and a number of others were working. Rather than just hollering and evacuating, he took the trouble to run in and out of each room, essentially forcing people out into the air, while he himself was ingesting it, breathing it. He lived for three or four days and died.

He'd had a grant from the Rockefeller Foundation to investigate the metabolism of carbon and what was suggested by someone (I don't know who) that Calvin take over the responsibility for that grant.

VM: Did that take place soon after he died?

SA: Yes, very quickly: days. I had been working in Hilgard Hall with a joint or paired committee of major professors, namely Gordon McKinney (who was a Brit but probably the best chromatographer in the business; he was involved in food technology in the department and the nature of flavours and that kind of chemistry)

and with Calvin. I was working on chlorophyll, structure — it had recently, in fact, just been synthesised by Willstätter in Germany and I obtained pure samples of it.

VM: Excuse me: this was as a graduate student?

SA: As a graduate student.

VM: And what had your background been as an undergraduate?

SA: Geology.

VM: So you were learning chemistry, were you, at that stage?

SA: Yes. Well, as I geologist you had to have chemistry but not the same kind of chemistry. I should stress that Calvin didn't know biochemistry either and so my having him and McKinney as joint professors essentially served as an introduction for him to biochemistry.

VM: And when was this?

SA: Where?

VM: When?

SA: When? '36, '37 on. I got my degree eventually in '42.

EA: Now when did Calvin come to Berkeley?

SA: I think a year or two before that.

VM: Calvin came to Berkeley in '37, I think. He was in England on a post-doc. with Polanyi from '35 to '37, and then he was appointed...

SA: But you see, he was teaching organic chemistry and although he had worked with some polycyclic compounds in England, I introduced him to chlorophyll. And so from then on we began to do a combined attack, if you will, on biochemistry.

VM: So you must have been the first, or one of his first, graduate students in Berkeley.

SA: Oh yes. There were a couple of others. One whom you will not meet, because he died, was Wayne Willmot: have you heard of him?

VM: No, I don't know about him.

SA: And there were a number of others: Lloyd Ferguson...

VM: Yes, we heard about him.

SA: What's happened? Have you seen him, heard him?

VM: No, we haven't seen him but we had lunch last Friday with Gus Dorough.

SA: Where's Gus?

VM: Gus is in Livermore.

SA: Oh, still there?

VM: Yes... and he's retired from the lab. but he lives in Livermore.

SA: He's nigh on to my age.

VM: He told us about Lloyd Ferguson and he thought that he was somewhere in LA.

SA: Well, he taught at the Los Angeles City College for a number of years. He disappeared.

EA: He got old, maybe.

SA: Well, but he disappeared before that; he just vanished and I don't know whether he became involved in something else or travelled overseas. His wife, you remember, was from the South and not completely happy with Los Angeles. She may have introduced him to some of the other possibilities in the South and he may have...

EA: Well, we always thought he didn't know he was black...until he went to that school; what was that school, that black school he went to teach at or something and he met his wife there and she was black, she was very black.

SA: Moorhouse?

EA: No, that's wrong. I think he went to Tuskegee for a short time just after he got his doctorate and he met his wife there. She was a black person from the South and he thought like Jesse Jackson, black. His wife never realised, really, that he was black and that it had any social significance until he went there because he was born and raised on the West Coast.

SA: Well, his pigmentation, mind you, was black enough but none of us ever thought in those terms — black, yellow, brown, whatever: it was meaningless. We socialised in his house and vice versa and there was never any problem.

VM: So all you guys, you and Dorough and Ferguson, were all Calvin's pre-war graduate students.

SA: Yes. Now with Sam Ruben's demise and Calvin's taking over of the Rockefeller money, the entire direction of research changed so to speak and in a sense that was the beginning of his activities so to speak. He had a consort, whom you've undoubtedly either met or know about, namely Andy Benson. Andy had had some difficulties during the war because he was a conscientious objector and was then rehired immediately afterwards but sought Calvin's understanding and the two of them formed a team. Andy frequently visited a variety of other labs. in order to see what was going on and he and Calvin would then discuss their direction subsequently. When I joined Calvin's lab., it was at the beginning of his receiving of the carbon-14...

VM: Let me get the chronology right. You were his graduate student from the late '30s until '42?

EA: Really McKinney's graduate student.

SA: No; I had dual major professors Mother, both McKinney and Calvin .

VM: And then what happened after '42?

EA: You were a postdoc. for years, though.

SA: Yes, until '43...

VM: With whom?

SA: ...with Calvin and then — Mother, you will have to keep me going on this — I'm a little confused on when we did the war work.

EA: You went off to teach at Boston University.

SA: Well, I took a trip across the country and went to Boston among other places. They needed someone to teach chemistry. They had contracted to teach a large number, two or three hundred of them, beginning chemistry and they urged me to please take this on so I did and I spent that year and a half, two and a half, year and a half (taught the summer session too), to an auditorium of soldiers.

VM: But you were not yourself drafted?

SA: No. I was urged by the Draft Board to take this responsibility as being more important than just becoming a soldier. After this teaching, then, and a bit of discussion with the President of Boston University who was not happy about some of my concerns with...

EA: I think they were the MIT connection.

SA: Well, you have to remember that President Marsh was a very conservative gentleman and his (*i.e. my?*) pay was in the same range— minimal. It was not enough to live on but I managed to eke out a living by going to MIT where they needed some help in spectroscopy. And so I did this: they had a contract with the army to test a variety of materials for transmissivity of various wavelengths but when the President's bagman, a Professor Alter (*spelling?*), became aware of this additional basis for income, they were quite upset and the President wrote me a letter saying how this must inevitably detract from my teaching and so forth and so on, and they both decided there was really no future in there. And so I left.

SM: Which years were these?

EA: '43, '44. And then you went off to Chicago to Franck's group for two years.

SA: That's right. On the way west, I was on my way home. But I stopped off in Chicago on the way west and there were a pair of gentleman, James Franck and Hans Gaffron, who were interested in a variety of things and discussed some of their problems with me, their chemical problems, and Gaffron was so impressed, I guess is the word, that he urged Franck to please hire me on the spot.

VM: What sort of area were they working in into which you would fit?

SA: Well, they were working on a variety of things but it was primarily carbon dioxide fixation.

VM: And had you had experience yourself in that area by then?

SA: Not really, no. This history of that briefly is as follows. It was done independently in three different labs.: by Wood-Werkman but that was non-photosynthetic so it was irrelevant. Then there was a fellow in Russia, who published an article in a journal *Biokhimiya*, in which he observed the phenomenon manometrically but I don't know that anybody has ever recognised it as being CO₂ fixation. He did.

VM: Do you remember his name, the Russian?

SA: Boichenko.

VM: Oh, that's a lady!

SA: I don't know.

VM: Oh yes, I've met her: she's a lady.

SA: Really? That's interesting because...you can't tell and I couldn't tell from the article and indeed when I wrote the thing up I put "her" in that gender but I wasn't sure that I was correct because, of course, in Slavic it's usually an "a" and if it's an "o" at the end you can't really tell.

VM: That's right but she is a lady and she worked in the Vernadsky Institute in Moscow and I visited her there.

SA: Well, give her credit. She was an independent and unrecognised scientist in terms of CO₂ fixation. And then third, of course, was Ruben, Sam Ruben.

VM: Did all these three know about one another?

SA: No. It's all independent. I think Sam may have known about Wood and Werkman but it was not photosynthetic, you see, and Sam's initial disclosure involved a finding that the radioactive carbon was in a molecule which he thought to be in the neighbourhood of 2,200 Daltons. It wasn't long after this...This was never...Yes, it was published. There was a publication involving it in the JACS but I'm not sure I recall when. In any event, this was of course quite incorrect; he gave it at a seminar in Chemistry which was well attended. Then, when Sam died, Calvin inherited everything and that was when the work on carbon dioxide fixation began.

VM: But presumably the work of Sam's that you describe was done with C¹¹, wasn't it?

SA: No. You see Sam was the first one to get C¹⁴.

VM: And he had enough of it at that time to use in that way?

SA: To play with, yes. To play with. He received the first amounts and he was the first one to look at photosynthetic CO₂ fixation.

VM: So Calvin, for all the contribution that he made, certainly didn't originate the idea of using C¹⁴ for CO₂ fixation?

- SA: Oh no. In fact, I don't like to denigrate him to the point that...but Calvin was a first class synthesiser but by and large the initial contributions came from elsewhere. For example, I don't know if you have Alan Brown on your list...
- VM: No.
- SA: Alan Brown was at the University of Illinois with Ed Fager, a physicist. They were going to give a talk at a Federation meeting on formation of sugar, six carbon sugar, and...
- EA: (*Inaudible*)
- SA: ...were investigating the question of whether it was a $3 + 3$ condensation or a $5 + 1$. They had come to the conclusion, in fact, that it was a $5 + 1$ and not a $3 + 3$. Now Calvin was under the assumption that it was a $3 + 3$ until Andy Benson, in visiting Alan, conveyed this idea to Calvin and then on the research changed. Now what Calvin did was to show, to develop the Calvin cycle; that was without question his most important contribution. He showed how the ribose, and specially the ribulose, could be regenerated from other compounds in a cyclical manner. But the CO_2 fixation *per se* was Alan Brown's and I don't think he's really gotten the credit for it. I'm not sure, in fact, that Calvin was even aware of the initial information: he absorbed ideas like a sponge and I suppose it's difficult to distinguish between original ideas...
- VM: Yes. Well, I suppose if Andy, in the course of his travels, had come across this and it had been a conversational item, it gets incorporated.
- SA: That's precisely the way it worked, precisely. And so, I had become familiar with chromatography and had introduced the notion of the chromatography of the radioactive products. I'll back track a bit on this.
- VM: Is this paper chromatography?
- SA: Paper chromatography.
- VM: You introduced paper chromatography?
- SA: I want to give credit first to someone else.
- VM: OK.
- SA: There was a young man in Biology at Berkeley by the name of Stepka, William Stepka: S-T-E-P-K-A. Bill Stepka turned up one day to the lab. and showed my assistant and me (Victoria Haas) this paper chromatography that he had invented. Calvin and Benson were using huge chromatography columns, you know, yea wide and yea long...
- VM: You are showing something three or four inches in diameter and several feet long.
- SA: Yes — and they wanted to isolate *the* compound in bulk, not with their approach, but Bill Stepka came up and showed Vicky and me the paper chromatography. I immediately saw how it could be used for detecting radioactive carbon and then we made some radioactive algae, I was growing algae for Calvin, and fed them some radioactive CO_2 : we isolated the sugar and not only did we chromatograph it but we

developed autoradiography because there were some big cassettes around in the Radiation Lab. at the time and I don't know what the Rad. Lab. had been using them for. But there were these large cassettes and I got some film and in fact we developed two-dimensional radio...

VM: And this was you who did this; you were the originator?

SA: Yes — but it was no big deal.

VM: No, but it is interesting to know who actually did it.

SA: Well, Bill was the originator of one-dimensional paper chromatography. What I realised were two things: first that two-dimensional would be better but that you had to use different solvents in the two directions otherwise everything would lie on a diagonal. And secondly autoradiography, and...so yes, that...

VM: So can I ask you about — by the time I got there some years later, of course, the thing was highly developed with all the equipment in place. What did you do about equipment? What did you use for two-dimensional chromatograms to start with?

SA: Ah! Well — as a matter of fact it was I who put them into the glass jars about yea wide and about that tall, put a bit of solvent at the bottom, rolled them into a cylinder and put them in. Then I ran them in one direction, took them out and to dry them and turned the thing and ran them in the other direction.

VM: I see: you ran them upwards and simply wrapped the paper into a cylinder and put it in the jar to develop in the solvent on the bottom.

SA: Later, later I developed the trough system and then we had the lab. build these troughs and we had little trons on top and put them in with a glass rod and ran them down, took them out and dried them and ran them again.

VM: And this was all your design?

SA: Yes.

VM: Good; I spent many happy hours with those things.

SA: No big deal.

VM: No but somebody has to do it in the first instance.

SA: Yes. Anyhow, what it did was to avoid this really heavy labour that Calvin and Benson were doing with the columns. Quick, simple and when we first showed...one of our first chromatograms, you know, showed a multiplicity of compounds far more complicated. On the other hand, if you reduced the time, which we also did, then we could show that the initial compound, in fact, that we could isolate from the CO₂ *was sucrose; we couldn't, we didn't see the ribose or the ribulose. (Note that the section in italics is missing from the tape.)*

VM: Could I take you back to get your times and whatever. You say after you were a post-doc. in Boston you went to work with Gaffron and Franck. How long did you spend with them?

SA: Oh, was it two years?

EA: You did three years.

VM: Two years. You came back to Berkeley then, in about '46?

EA: Yes, that's when Glenn Seaborg came back.

SA: Glenn Seaborg was working in both labs., in the Radiation Lab. at Berkeley and the one in Chicago, and on his way through the Chicago one he stopped in to say "hallo" and I pled with him to get me out of that...

EA: Germanic atmosphere?

SA: We did not get along, Professor Franck and Gaffron and I. In all fairness, they weren't happy with my point of view about a variety of things. They were distinctly Germanic and I think that (as long as we're writing history we might as well be truthful about it) he and Rabinowitz, who was at Illinois, were once speaking in the hall, in German which I understood perfectly well, saying that they thought they would develop Germanic education in the entire US graduate schools. This would become a model or a focus for...they weren't happy to have anybody listening in on this. And so...

VM: So you came back to Berkeley in about '46; and what were the circumstances in which you were able to join Calvin's group?

SA: What do you mean by "the circumstances"? I asked "could I join?" and he said "sure". Money was no problem.

VM: He gave you a job?

SA: Yes.

VM: You were an employee of the Rad. Lab. or whatever?

SA: Yes.

VM: On a permanent basis?

SA: Well, there was no end in sight.

VM: I see. And by that time, who was there? What constituted the group?

SA: Oh, there were quite a few.

EA: Bassham? What was his name?

SA: Alan? Alan wasn't really part of that group. He was working on...John Weigl and Alan Bassham were working on their Ph.D. theses but I don't think it was related to...

VM: He was working on succinate, on dicarboxylic acids.

SA: I don't know what he was working on but I don't think it had anything to do with that group.

EA: (*Indecipherable* — *White?*) was there, wasn't he, at that time?

SA: I'm a little bit confused as to the initial time and the subsequent time.

EA: We all played bridge at noon.

SA: Ah. That was — was that in '36?

EA: I think that was in '46; '46 to '47.

SA: Was that the bridge school? Melvin was unhappy. He would come down at noon and see us playing bridge and would do his utmost to...

EA: ...break it up!

SA: He was a verbal whip and say you mustn't waste your time wasting your time that way when you should be at your lab. bench making big discoveries. He asked Lloyd Ferguson and Wayne (*indecipherable*); you know, if you were to name the others I could remember them but I...

VM: Well, I'm not sure who was there at the time. I guess Andy was there.

SA: Andy was not...

EA: ...a bridge player.

SA: ...involved in our...Andy stuck pretty close to home meaning he did his bit of teaching and he made his journeys around the country to find out what other people were doing.

VM: Well, the other people that I know were there: there were Donner people — there were Dick Lemmon and Ed Bennett.

SA: Dick Lemmon and Bennett were over in the other building.

VM: Yes, in Donner. They weren't playing bridge?

SA: No.

VM: Really, who was there? Was Murray Goodman there at the time when you were there?

SA: No; doesn't ring a bell.

VM: I'm pushed to know who was there, actually, at that time.

SM: I remember hearing about this bridge game but I can't remember who from.

SA: Gus was not involved, Gus Dorough wasn't involved. But Gus would know if his memory is better than mine.

VM: Well, we could call Gus. Do you remember a guy called Elmer Badin?

SA: No.

EA: No.

VM: Because I don't have a good chronology of when everybody was there. Not yet — Marilyn, Marilyn Taylor is getting it for me but she hasn't done it yet. OK: so you came back as a regular staff member in the lab.?

SA: Yes.

VM: And you worked in ORL?

SA: Yes. Never up on the Hill.

VM: Right. So all the things you developed down there, the chromatographic and the radioautography and the...What about the counting stuff? How did that come about?

SA: Well, the way we first did it was to have a little counter tube and we'd stick — I did the autoradiography so I knew where the spots were — we'd put the tube over it and count it.

VM: And these were conventional commercial counters with mica windows?

SA: Yes; from Nuclear Chicago. And indeed they were so pleased with it that I recall that they invited me to come to Chicago to see their newer equipment and I was there for three lovely days and saw my first night-club events. Who were those...?

EA: (*Indecipherable*)

SA: No, no, no! They were three men and a lady and I heard them perform not too long ago.

EA: You don't mean Peter, Paul and Mary?

SA: Yes. Two men and a lady; Peter, Paul and Mary.

VM: Peter, Paul and Mary were your first night-club entertainers?

SA: Yes!

VM: Where had you grown up?

SA: Berkeley. But it depends on what you mean by "grown up". I was at university in Los Angeles but I couldn't stand LA.

VM: You went to high school in Los Angeles?

SA: Yes. Venice High School.

VM: Venice?

SA: Yes. Well, if you're familiar at all with LA...

VM: We have a son who lives in Los Angeles.

SA: ...there used to be a large fountain in front of the... Venice High School doted itself on art. The major instructor there felt that art was so necessary for human existence that he had a huge fountain built in front of the school and at the very head end of it was a statue of a lady, a nude lady, young lady, turned out to be Myrna Loy the actress who had been a student at Venice High and this art instructor was so taken by her lovely figure that he had carved a statue of her and made it into a fountain.

VM: And you stayed there until you went to Berkeley as an undergraduate?

SA: I went to UCLA...

VM: I see, yes.

SA: ...which was most unhappy...

EA: It was very new and (*Indecipherable*)

SA: They had just moved from downtown...

EA: They had a teachers' college mentality — still.

SA: Yes...

VM: That's how UCLA started, was it, as a teachers' college?

EA: You see, it was a teachers' college downtown and then...

SA: ...and they were given land out in Westwood.

EA: What was the name of that developer?

SA: Well, it was in Westwood but that wasn't the name of the guy.

EA: He had all that land, he bought up all that land, and he donated...

SA: ...the land to the university so he could develop Westwood.

ES: Yes: then he developed Westwood.

VM: Ah, clever, yes!

SA: It worked., it worked. In any event, the university at that time was not only immature in terms of teaching but as history showed it was a hotbed of Nazism and indeed much, all of the faculty in German language were fired because they were so anti-Semitic and replaced so that it was unpleasant, it was unpleasant, and I did not do well at all. But there was a lovely teacher in geography; in fact, I did so poorly that I was on probation and the head of the committee on students who were on probation was a Professor MacDonald who taught geography — a very kindly-looking old lady and when she spoke with me she said that I was probably in the wrong area and would I take her geography course so she could evaluate me? And I took her geography course and did quite well as a result of which she wrote a letter to Berkeley to the office that provided employment for students and said "please give this young man a job".

So when I got to Berkeley I was able to go to seismology and was given the job of sorting out a charcoaled piece of paper with a needle scratching through the charcoal and my job was to change it every day and file it. I got into the basement where this seismograph was and found an absolute jungle of records that had been dated but not stored in any way. So my first job was to organise these and it took quite some time to arrange them in sequence and I then took care of the seismograph and noted when there were...

EA: Significant changes.

SA: ...events. Yes.

VM: Were you being a student at the same time?

SA: Oh yes, of course, that was how I was able to live because the teaching assistantships were very few in number and generally given only to students in their final year. This was a very difficult thing. As a result of Professor MacDonald, I had this job which saved me — at 25¢ an hour!

EA: Yes — it was big money!

SA: Well, it was enough for a very healthy bowl of soup which my wife and I would eat for dinner at a Chinese restaurant.

VM: You were already married at that point?

SA: We married. Yes, indeed, she came up with me.

EA: There was a little lag time.

SA: Oh, we've been married now sixty years and so...

EA: But you must remember that he graduated from high school when he was fifteen so he went on into UCLA at a very young age and I always felt that that accounted for his doing poorly. He wasn't mature enough yet to settle in.

SA: Well, there were fifteen of us who were fifteen years old at the time and I was the youngest of the fifteen. So she may be right; we were certainly conferred no advantage.

VM: But by the time you went up to Berkeley you were already married?

SA: Yes.

VM: So you must have been married pretty young?

SA: We married...

EA: I was twenty.

SA: I don't recall whether we actually got the marriage certificate...

EA: We got married in Berkeley.

SA: Yes, I thought (*indecipherable*) in fact or in Oakland.

EA: I think there was a lag time. I think you went up first and then I...

SA: Yes, you're right, quite right Mother, you're quite right. And I think the actual marriage certificate was issued from Oakland.

EA: Yes.

SM: And which year were you married?

SA: '36.

EA: '36. I was born in '16 so twenty was right.

VM: OK. So as somebody in the lab. when you rejoined it in '46 and you had a job with them and you worked in ORL, you said there were several people there working in the group at the time.

SA: Yes, and apart from Ed (*indecipherable*) I'm darned if I can remember who they were.

EA: Wayne (*indecipherable*).

SA: Yes, Wayne didn't — Wayne was in the lab., in and out for a while. You see, the war was beginning...

EA: ...to wind down.

VM: In '46 it was over.

EA: Not with Japan, I don't think.

VM: Well, the bomb was August 8th, '45.

SA: We were working on cobalt...

EA: yes, of course.

SA: ... compounds which would combine with oxygen in the hope that they could be put into submarines and which would provide a source of oxygen in the event that the oxygen tanks had to be thrown away; because the additional speed that could be attained by that was meaningful. On the other hand, they were then bereft of their oxygen supply underneath so this ability of this cobalt chelate to assimilate oxygen reversibly meant that it could conceivably be a source for them.

VM: But you weren't doing that, surely, when you rejoined Calvin in '46?

SA: Oh yes.

VM: You were still working on oxygen chelates?

SA: Oh yes.

- VM: I see. So you weren't totally dedicated to photosynthesis when you came back to his lab.
- SA: Oh no.
- VM: But you were nevertheless a participant because you designed the chromatography...
- SA: Yes, and we grew the algae. My official job was to grow the algae.
- VM: When you say "we" ...
- SA: Vicky and I. And I developed a tank with fluorescent lights which were fairly novel.
- VM: And those flat-bottomed flasks, one of which still survives?
- SA: Well, that was my design.
- VM: Yes.
- EA: When you were with Franck you were working on photosynthesis, too.
- SA: Oh yes. But that was in a very different way. Franck was not happy with that — I sort of had to do that on the side in a sense. You see, Boichenko had developed this manometric method for CO₂ fixation that was not known. I mean, you had to be able to read Russian...
- VM: And you didn't know of it?
- SA: I did know; I found it in the library and I immediately saw its virtue and so I resuscitated some manometers and all of my work with Franck and Gaffron was manometric.
- VM: Do you read Russian?
- SA: I did at the time.
- VM: So your work with Franck and Gaffron was all manometric following Boichenko's example?
- SA: Yes.
- VM: But when you went to Calvin...
- SA: I spent much of it setting up the lab. You see, there had been no fellow there before me — (*indecipherable*) Davis; I don't recall his name any longer but he was unhappy with a number of problems so I inherited his lab. and spent a good deal of time cleaning it up and getting the manometric equipment to function so that was the basis of my activities there.
- VM: How long were you there roughly?
- EA: Three years.
- SA: Yes, three years. Three years of Chicago is five years of anywhere else.

EA: Very urban.

VM: So when you came back to Berkeley, to Calvin and you were part of your time working on the cobalt oxygen chelates and partly you were working on photosynthesis. And the big breakthrough was the arrival of Stepka with the chromatographic paper...

SA: Yes.

VM: And you were the one who began to run with that first and to develop the thing?

SA: Yes.

VM: And how far did you get? How long were you associated with the work after the chromatography started?

SA: Oh gosh. Where did we go after that Mother?

EA: We went to Iowa State in '48.

SA: I was wondering whether — yes. There was a fellow in agriculture — I think I mentioned his name: J.P. Pennant — an elderly gentleman who...

VM: That was in Berkeley agriculture?

SA: Yes, from Missouri. Who had become aware of this position that was available at Iowa State and asked if I would be interested. It involved radioactivity because there was an institute for atomic research there headed by a Professor Spedding (*correct spelling?*). Spedding and his group had developed the material for the bomb. And in fact, not only developed, it was produced there. And so I was asked would I be interested in joining the group as a biochemist and I said I would. When I got there (I had been preceded by a certain amount of bargaining) and the Dean of Agriculture was interested in introducing the use of radioisotopes into agricultural research so I was put into Botany much to my...

EA: Horror.

SA: ...disgust and so I spent the initial years in Ames in the Botany Department, complaining all the time that I taught plant radiobiochemistry and I must admit I learned a lot; I had small classes because there were not many interested in it and had ample opportunity for research.

VM: So that's why you left Berkeley then in '48 in order to take up that position?

SA: Yes. It was a tenured position.

VM: So by the time you had left, had there been much progress made with the radioactivity and the chromatography that you had introduced?

SA: No, not much except that I had convinced Calvin and Benson to go in that direction. So they gave up their columns then and went on to paper chromatography themselves after that. It was the obvious way to go.

- VM: Do you remember anything about your social life in Berkeley? (*This question was not recorded on tape.*)
- SA: Occasionally we would go out and play soccer, or touch football it was, touch football.. And I remember Lloyd saying that he didn't realise that as quiet a person as I was in the lab. could be so aggressive on the field. But that was the extent of our social activity.
- VM: Did you not — well I guess you were married, and maybe some of the others weren't, so you had a domestic base; did people not tend to act in a social group, people in the lab.?
- SA: I don't understand what you're...
- VM: Did they spend their spare time together, did they socialise out of lab. hours?
- SA: I wouldn't know.
- VM: You didn't?
- SA: No.
- VM: Did they have things like Christmas parties or trips to the mountains or that sort of thing?
- EA: That's due to institutional evasion in his head.
- SA: It just wasn't done in those days.
- EA: Well, it wasn't institutionalised. (*Passage here indecipherable*) ...and they worked late and independently and it would be difficult to have sort of social routines.
- VM: Right.
- EA: You know, it was in transition, Berkeley was. Before the war it was kind of a family institution vaguely. You know, you were part of a family. It was big but it was still family, you know, still people-oriented. But after the war they got these big grants and it turned into an institution. And so this was the end of the era, '46 to '48, before it became big business.
- VM: I see.
- SA: I'd have to ask you something — she reminds me. How did Koshland Hall get named? Did they — Dan Koshland was a staff member.
- VM: I don't know. The short answer is that I don't know. There is a plaque in the hall which has got his picture and...
- SA: Dan Koshland's picture?
- VM: Dan Koshland's picture.
- SA: His family must have a tremendous amount of money.

VM: I think that may be the case. You know that he's Levi jeans?

SA: No.

VM: Does that answer your question to some extent?

SA: That tells me how he...

EA: ...where the money came from.

VM: Well, where it *may* have come from.

SA: Yes.

VM: Yes. I think that may be the relationship.

EA: There was a fellow — was he down there? There was a fellow whose family were in politics in Los Angeles. I think his name began with a "Y". I don't think he was part of that group in Berkeley. I think he was always...

SA: No, but there was a graduate student, Ralph Yount, who went to Pullman. He had nothing to do with our group.

EA: No, no. I think that the one I'm thinking of never left Los Angeles. I think you knew him down there.

VM: So after you left and you went to Iowa, did you continue working in photosynthesis?

SA: Oh yes.

VM: What was — in the same sort of way, in radioactive carbon?

SA: Oh yes. Of course, much of the field had been exploited so one could only do accessory things.. One of the things I did was to develop a method for degrading glucose to show the distribution of radioactivity within the molecule and thereby imply its mechanism of formation.

I think the most meaningful experiments were in transport. I think it's been assumed then, and probably rationally, the sugar made in the leaves was transported in some form down to the rest of the plant but my experiments were the first to actually show the transport. In other words, fed the C^{14} to a small area of a leaf and show how the sugar moved out of that area, in fact, to show indeed it was sucrose that moved and that it was transported from the leaf to the rest of the plant. Anybody who's ever gotten sap out of a tree would say "so what?" But this was actually the first demonstration.

VM: So from being an original geologist through a chemist and a biochemist, you became close to a plant physiologist? And is that how you developed the rest of your career, in plant physiology?

SA: No. There was a gentleman from MIT by the name of Bear, Richard Bear, who took over as a Dean of Science for a year and who was aware of my complaint of residence in Botany. In all fairness, they had done all they could to make me happy there. They had rebuilt the lab. in Botany, made it specially suitable for work with radioactivity

because there were faculty members in the department who were concerned about my use of radioactivity and that it would affect them in some disastrous way, so they were actually anxious to get me out.

VM: This was in Iowa?

SA: This was in Iowa State and so this lab. was built for me to allay their fears. But Bear saw the virtue of developing a Department of Biochemistry and he, in fact, initiated this department. I was one of four or five original members and we were stationed in Gilman Hall in the Chemistry Building. So I moved out of Botany at that time and Spedding, Frank Spedding, who was Head of the Institute for Atomic Research, then rebuilt part of Gilman Hall for use in that manner.

VM: But you didn't stay at Iowa State very long, did you?

SA: Twenty years.

VM: Twenty years, you said; oh really, twenty years?

SA: That's not too long.

VM: I'm sorry. So that was taking you now to the '60s, the late '60s. And where did you go after that?

SA: Well.

EA: He was seduced.

SA: There was a Professor of Economics who had a son in the west and who somehow arranged to get hold of the administration at Simon Fraser University and I received a phone call...

EA: You went to Boston College for two years.

SA: Oh, excuse me. Yes.

EA: As Dean of — what was it? Science?

SA: I was Graduate Dean and Dean of the Faculty of Science.

VM: Boston College was in Boston, was it?

SA: Oh yes, and it's a Jesuit college. And we were never really quite sure why they wanted a Jewish lad to be their Dean of Science and so forth but it was never a problem. Never. They had taken down all of the religious items in the room, and you could tell from the shadows on the wall where they had been, but we were on the best of terms. And so, yes, I spent a couple of years there although I felt that I was really not quite part of the scene.

You know, in Boston you are not...you are a newcomer throughout your life, regardless of the length of time you spend there. Edith and I felt that it would be at least two generations, probably three, before you were accepted as belonging to the area and so when I received this phone call from out of the blue asking whether I would be interested in coming as a Graduate Dean to this new university in Burnaby,

British Columbia. I was aware of Simon Fraser as an explorer; I didn't know that he had this university named after him. But Edith and I went out to check it out and decided to go there.

SM: This was in 1971, was it?

EA: Yes, '71 I think.

VM: And you stayed there for the rest of your academic career?

SA: Yes.

VM: And have you retired from it now?

SA: I've been retired. They changed the retirement rules after I left but if they hadn't I might still be there. You had to retire at 65.

EA: You retired at 67.

SA: Yes, I talked them into the additional two years.

VM: Well, I was part of an analogous system and I had to retire at 65. By the time I retired they were no longer interested in being talked into 67.

SA: You could only work part-time after that. In decreasing years, so that by the time you were 72 you could only teach a summer session.

VM: Oh I see — and they faded you out.

SA: Yes.

VM: It was like old soldiers fading away.

SA: That's right.

VM: I think it's time to turn the tape over actually. So thinking back now after all these years — after all you left Calvin in something like '48 or so, that's a long time. How does the whole activity strike you now, looking back on it and what a group like that was able to achieve in the fifteen or so years that followed?

SA: I'm not sure what you're asking. I think there are many problems which are so complex that they have to be solved by groups rather than an individual doing it over a lifetime. In those days, this was the simplest approach: Calvin had a very good set-up and so the lab. work, I think, was highly successful and the group was congenial so that one could ask no more. I had no complaints whatsoever about that period.

VM: Do you think it was the sort of work, the sort of exploration and development, which required unusual understanding and perception or once the initial ideas were in place was it reasonably straight forward and all sorts of people might have done it?

SA: Oh, I think science is sufficiently logical so that if one is well-trained there are obvious paths to follow. One of the paths which we didn't follow, for example, which Calvin was able to perceive immediately when I thought of following in that direction, was to do the crystallography of the cobalt compound. I had learned how to

crystallise the material and had taken a microphotograph of some of the more perfect crystals and when he saw them it was quite clear to him, as it was to me, that would be the next stage to follow so that one could have detailed knowledge of how the cobalt and the oxygen combined — what kept it. That may have preceded some of the work that is now routine but it wasn't in those days. So that I think that well-trained minds — and we were well-trained — have an obvious scenario.

VM: So once the original steps had been taken, really by Ruben in the use of radioisotopes to follow carbon fixation in photosynthesis, the idea was already around and available for people to run with.

SA: I think so. Mind you, there was a tremendous amount of work to be done...

VM: Of course.

SA: ...and it was not common in those days for chemists to work with living material, biochemical material, but they were getting into that system. It was, in fact, the beginning of real biochemistry.

VM: It's interesting that none of the plant biologists, or botanists in those days, actually did it: it was the chemists who did it.

SA: That's right.

VM: The botanical ideas of photosynthesis at that time were very primitive — of the carbon pathway, fixation.

SA: Oh, I would say they were essentially unknown.

VM: Yes.

SA: As I mentioned earlier, what was being done in those days at the time by plant nutritionists were to ascertain the nature of the elements which were required simply for plant growth.

EA: (*Indecipherable*) ...Fay Morgan...in some little venture. I don't remember what it was.

SA: Really, I don't know about that.

EA: Yes he for a while was...

SA: Agnes Fay Morgan was an economist, a home economist.

EA: A nutritionist, I'd forgotten about that. But you see I was taking some courses in epidemiology and the best epidemiologist was in this home economics set-up at that time at Berkeley. She, by the way, had only a bachelor's degree but she was a full professor. Veedy (*spelling?*) I think the name was and a leader in developing epidemiology which just shows that (*indecipherable*) a doctorate. Yes, but you see that Berkeley as a whole, again, was a family — you know, the people mixed. They weren't compartmentalised as much as I think they are now.

SA: Well, Berkeley revolved around some institutions. One prominent institution was the Faculty Club. Business of all kinds was done at the Faculty Club and if you wanted to get something done that was where you went if you were allowed in. The centre of

much of the activity was the bridge game and the bridge game was dominated by G.N. Lewis and Gerry Branch and whoever else they might invite to play.

VM: In the Faculty Club?

SA: In the Faculty Club. I don't mean this in a demeaning way; it was simply a social atmosphere for much of what went on. My benefactor was the head of Botany.

EA: Hoagland.

SA: No — Hoagland was Plant Nutrition. And for the moment his name escapes me. I was a reader in Botany, meaning that I read the examinations, and one day he received a complaint about my grading so he asked me to come in and explain to him how I had come to this grade. I showed him the examination and how I broke down the answers into parts and the numbers which I assigned to those parts, and he was thrilled that somebody had taken the time to be that concerned with grading and thereafter I could ask anything of him, so to speak. But he was responsible for my getting a Teaching Assistantship and for showing my value to chemistry and all of that.

VM: Well, I think that's a very interesting story that you've told us. We had not been able to go back quite that far before we talked to you.

SA: Indeed, it goes one step further: I almost became the Provost of Santa Cruz — but that's another story.

VM: OK. That is for the next set of researchers. Thank you very much, both of you: we very much enjoyed being with you.

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BIOGRAPHICAL INFORMATION

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PhD (Berkeley) Physico-Chemical Biology
Occupation(s) Prof., Administrator (Grad Dean,
V.P. Research)
Areas of expertise _____

Other interests or activities _____

Organizations in which you are active _____

Chapter 25

RODERIC B. PARK

Berkeley, California

July 9th, 1996

VM = Vivian Moses; RP = Rod Park; SM = Sheila Moses

VM: This is a conversation with Rod Park on July 9th, 1996 in Berkeley, California. OK — all yours, Rod.

RP: I got my PhD from the California Institute of Technology, CalTech, in the spring of 1958. I served as a postdoc. there for a short time in geochemistry. I visited the Calvin lab. and Calvin had pointed out to me that Ozzie Holm-Hansen would be leaving the lab. that summer and he needed someone to take care of his algae lab., probably more correctly called algal lab. (if it's an adjective). But it was called the algae lab. in the Calvin days and located in the upstairs of the Old Radiation Laboratory. I arrived about July 1 to start work in ORL and my wife and kids and I lived for a while in a motel down on University Avenue while we were searching out a house and I used to take the bus up to ORL.

I saw Ozzie Holm-Hansen in his last few days who he seemed to be particularly happy and somewhat blithe about leaving and was rather inexact about all the problems and all the things I was going to face. So, I got started and one of the first things that happened was it seemed that some of the cultures might actually be contaminated with bacteria and were not actually pure cultures. I went back to single cell isolation and one of the things that had been mistaken for contamination was that the cell walls of *Scenedesmus* stained like bacteria and people had mistaken these. I think they were contaminated. We went back to single cell isolation and worked up from cultures and eventually got both the shake cultures and the continuous cultures. Operating this took a considerable amount of time. We also set up a pump that had parallel pistons for air and for CO₂, to generate a large CO₂ supply so we weren't always twiddling the dials. We gradually got that under control.

VM: Can I ask you two questions. One of them: was this the older shake flask arrangement or the vertical?

RP: We had both the old shake flasks as well as the continuous culture, the vertical ones.

VM: And who was the other half of "we"? You said "we".

RP: Well, Pat — what was Pat's last name?

VM: Pat Smith?

RP: Pat Smith was there...

VM: ...the redhead.

RP: ...the redhead was there and I inherited a staff member who had very good intentions but, I think, a lot of instability psychologically in various ways. That created its own dynamic of certain difficulties in getting this whole operation going. We finally got it under control. My office was right around the corner, just to the north-west of the lab. beyond the stairway, which turned out to be E O. Lawrence's former office in the Old Radiation Laboratory. He actually had an office through a door just to the east that went out under the roof from my office, where he had a desk, and he actually came in once during that fall and I had an opportunity to meet him and went to his desk in the other room. Later that fall he died. But I did have an opportunity to meet him a month or so before he passed away.

The science was interesting. I saw Calvin and he said for me to get the lab. started and see Vivian, and see others, and Ning Pon, run some chromatograms and find out how to do the carbon cycle work.

VM: I can't remember whether you actually arrived before I left, but you might remember.

RP: Yes, I arrived before Vivian (*sic!*) left by a couple of months. I only spent about six months in ORL — all of us did — and then we moved down to the Life Sciences Building as an interim move before the Round House was built. I had a remarkable industrial strength experience with Melvin about three weeks after I got there. I had not run chromatograms; I had been isolating chloroplasts. I was interested in some geochemical work on what the fractionation factor would be — we never got this completed — but the idea was I wanted to find out what the fractionation factor was in photosynthesis for the O^{18}/O^{16} ratios of oxygen produced as compared to the water source of the oxygen, because it wouldn't be exactly the same; it would be a fractionation factor.

I started working on this and started working with chloroplasts and started working with Ning Pon on CO_2 fixation by these chloroplasts also. We had done some experiments that looked kind of interesting in terms of what the membranes did and what the supernatants did from these chloroplasts, and which activities the carbon cycle were held and which ones, which, of course, is in the supernatant was the membranes being responsible for electron transport and phosphorylation. We had done some preliminary experiments and Calvin came up at the end of the day, it must have been about 4:30 one afternoon. I was alone up in the algal lab. and he said to me, "well, have you been running any chromatograms? What have you been doing?" I said "no, I haven't been running any chromatograms, but let me show you what I have been doing." He got quite angry, and started asking me all sorts of questions because obviously I hadn't done what he'd asked. I kept holding my ground and showing why I thought this was interesting and why I thought it would be an interesting direction to start working in. He was really roaring by the end. I sort of felt well, I've been here about a month or so and maybe I'll be leaving next week! I thought maybe I was out of there. I went down and my wife had come to pick me up in the car and I said "I'm not sure how much longer we're going to be here." The interesting thing was, he never did that again. As a matter of fact, he was very supportive of what I was doing. Apparently, I don't know whether consciously on his

part, it had been a test to find out what I was made out of. It was rather remarkable to be a wet-behind-the-ears PhD recipient challenged this way by a Nobel laureate. It was something I will never forget. On the other hand, I was sure that I was doing good work, I wasn't about to let him roar over me without listening. Apparently, he did listen because after that whatever I was doing he was very supportive. He gave me my own head and let me go. I must say that was a remarkable experience. It's the one that most sticks in my mind from that whole lab. experience as being one of those forks in the road that happen in your life.

VM: I'm not sure that I can recall, maybe you it said already, how did you first come to meet him?

RP: I came to meet him on a trip up here because, as a graduate student, I had started working with James Bonner (*at CalTech*) on terpene biosynthesis and eventually in the rubber plant. Of course, we were doing me-tooism in plants because it had been worked out in liver. We started out feeding acetate labelled and that worked fine, we could degrade the rubber, it went in the right way, but all the intermediates he proposed didn't work because it was laevulinic acid and we were trying β -methylcrotonic acid and various other things which gave randomly-labelled rubber. Bonner sort of took this view that you were against me when it didn't turn out right. It was kind of upsetting!

So I went over at that time at CalTech and started working with the geochemists (I had done a minor in geochemistry there) particularly on carbon isotope fractionation. We did some control experiments on carbon isotope fractionation in photosynthesis and that got me interested in photosynthesis. And, if there was a place to work on it, it seemed to be, particularly with respect to carbon, it was Berkeley so I took a trip up here and met Calvin, told him what I was doing and told him I was interested in coming up. He called around to a few people, I guess, and offered me a job as a Chemist P1 which was the lowest level that you could possibly get in the Lawrence Berkeley Lab. at the time.

VM: But that was as a regular employee?

RP: Regular employee.

VM: And it was an open-ended thing, it wasn't time-limited?

RP: It wasn't time-limited but we really didn't discuss it. We assumed that it was a year or two, something like that.

VM: What impression did he make when you first saw him? Presumably you knew about him and knew what he was doing?

RP: My impression was...I met with him in his old brick office — I guess it was G.N. Lewis' office at one point in the Old Chemistry Building and that, of course, had its own aura of tradition, of vast glass-fronted bookcases and fireplace and all these other matters in it. I found Calvin himself kind of hyperexcited about things, a man who was so intense he obviously had at least at that point in dealing with me, no sense of humour whatsoever. It was all extremely...well, I guess the word is intense and excited and petulant. At times he could be very petulant, didn't like you to disagree with him, although he would immediately create arguments, but he got a little petulant...

VM: And if you were winning the argument?

RP: I had that time, once. I think I won it, but he didn't acknowledge it until about a day or two later. I think he thought things through pretty well and had his mind kind of made up and it took a lot of thinking on his part to change it. That's the way it should be. One shouldn't be ephemeral about these things. That's certainly the way he was.

I was with those first six months. I started very satisfactory cooperation with Ning Pon, which was a lot of fun, we had a good time together and did some interesting work. We moved after six months to the Life Sciences Building, the old Greenberg space. You weren't there then.

VM: No, I'd gone. What was it like in LSB? We haven't actually heard from anybody what the move was like, what the mood was like, what the mood in ORL was like as it came to an end and things like that.

RP: The move went off, I think, smoothly. The space was behind time down there. I remember they had a budget which sounds ridiculously low now, it was \$75,000 to remodel the space. It was in lower floor, the north-east and along the northern side of LSB, which is now occupied by the Jepson Herbarium, down in the basement there. I had what had been an old teaching lab. as part of the medical school, before that moved to San Francisco, for the algal lab., with benches taken out on one end, with all the algal laboratory itself for the growing facilities, and then several lab. benches for us to do our work on. There were other labs. around the way, some of them were Rapoport's, various other facilities. The next lab. to the west, the big double lab., was made into one large room, at least it was connected at the end, to try and at least get some of the large lab. interaction and experience that was done in ORL and was again done in the new building. Obviously, with walk-through corridors and all the rest it did not have the same sense of family that ORL did, or that the new building does. It was an interim solution. I can't remember how long we were there, a couple of years.

VM: Oh. actually from the end of '58, I think, until '63.

RP: '63; five years, yeah. I was involved in the design and construction of the new building as part of the building committee. Some interesting things happened there. The architect was Michael Goodman, who was on the faculty here, a typical inside job (I don't think they are done that way any more) but you sort of take care of your own in architecture and so forth. Calvin had a distinct idea of what he liked. He wanted a building where people were forced to interact with each other, interdisciplinary building, and that's why it eventually ended up round. It kind of focused everyone at the coffee table or something with special facility rooms in the back. We had our first meetings and (*Calvin*) said what he wanted. Louis DeMonte was the campus architect at the time and Michael Goodman understood what Calvin wanted. He first started drawing a square building, which sort of forced people to interact, and then he drew a round building which had interesting design characteristics. If it's smaller than a certain size, you can't have interior corridors; if it's larger than a certain size, you run into all sorts of problems also. It turns out that there are certain quantum sizes of round buildings that make them work well for science. So, Calvin's building — Michael Goodman came in with this round design.

The campus architect, Louis DeMonte, was absolutely shocked. He said, "Michael, when that goes to Sacramento we could lose every building in the building programme of the University"; he got really angry at Michael Goodman. Calvin kind of sat there and listened to it, didn't say anything. We had a meeting again about ten

days later. Calvin had obviously gotten to the important regents at that point. Louis DeMonte said “well, I think a round building might be rather attractive statement right at this point on the campus.” I saw University politics operating! I think (*Donald*) McLaughlin was probably the regent who pulled a turn on what kind of building Calvin could have.

VM: In the early days of the design, from what you say, it seems that Calvin had been able to impress upon Michael Goodman very much the sort of building he wanted, or at least the properties he wanted the building to have.

RP: He impressed on him the programme, before the design, of how he wanted people to interact. That was the (*most important thing*).

VM: The design of the building came from Goodman and from the (*campus*) architect and not from Calvin himself.

RP: That’s correct. Calvin thought the round building might work...

VM: Had he?

RP: Yes...and in Calvin’s mind it was a replacement for the old Bacon Hall which was a museum which used to exist down right to the south-east of the Campanile — that would have been torn down for the new physics building — and Calvin also thought of it as a replacement for Bacon Hall in terms of that kind of architectural statement on campus. (*Note: Bacon Hall was a semicircular old red brick building, one of the original campus buildings.*)

VM: Bacon Hall was geology, was it?

RP: I think it was geology. It had a geological museum in it. I just went in there to look at the rocks. I never looked around at the building. You notice, one other thing that happened was that Calvin kept saying, “well how do we connect the two floors?” And he put in that little spiral staircase so that you didn’t have to go to the other staircases. That was a hell of a fight with the fire marshal and everybody else as to how you connect the two without having everything locked-off through doorways. But somehow they got it done.

VM: At which stage do you remember did they start looking for money? Because clearly, they weren’t going to build a building unless they had cash for it. They had to go out with some building proposals.

RP: The building as I recall — again it seems incredibly cheap by today’s standards — I think was two million dollars. Calvin had raised quite a bit of money, I think, from the Health and Human Services people, which was HEW at that point — they had given a big grant and he had gotten several other grants and private gifts. I don’t really recall the details of the financing but I know that he was active in fund-raising. I was not involved in that in any way but he would speak about it at his lunches over in the Faculty Club, in the Directors’ Room where we would meet every Thursday noon for lunch.

VM: Had he got the money before the building design was finalised?

RP: I think they were going on simultaneously. He had enough in-hand to get going and I guess The Regents felt that there was a good possibility he was going to raise the rest

of it. So it was not totally in-hand. He was still raising it while the whole building planning and design was going on. He certainly had enough to cover the architects' fees and the 10% at that front end, and then it just kept on coming in.

VM: Was there discussion in the lab. generally about what the building was going to be like or was it confined to the Thursday lunch people?

RP: My interaction with the design was primarily through the Thursday lunch people and Calvin and the architect and the building committee. There was discussion with others but I don't remember there being strong opinions about it from the transitory people who were there for a year or two. It was really kind of Calvin's show and the more permanent staff's show — people like myself.

VM: What's your own view? Do you think that ORL was the glory building in which regard that some people hold it?

RP: I think it certainly had some great science going on in it. I think the architecture sort of by accident contributed to that. It's not a building that you would design and reconstruct. On the other hand, it had this fortuitous configuration. I think the...Calvin obviously wanted to recapture that. I think part of the sense of morale that existed, which seemed to be pretty good in ORL, had something to do with the fact that it wasn't such a hot building. I think when you get a building that is sterile and artistic and totally finished, sometimes it's — maybe I'm speaking about myself now; I think I'm speaking about myself — it's harder to focus in those surroundings, sometimes, than it is in surroundings that are a bit more demanding. Sometimes surroundings that are a bit more demanding for me generate a greater sense of creativity than in the building that seems totally finished and perfect.

VM: Well, you grow into the building; the building is built around you, grows around you.

RP: Yes, it grows around you and maybe it's a building that's not finished, that needs improvement, to me leads to greater creative energy on my part than one that's absolutely finished and perfect where you don't have that sense that things aren't right, let's make them better, that may just infect you.

VM: I think the personalisation of a building, the fact that you have the stain on the ceiling, you remember the time when somebody shot something up there, it makes it real for the people that live in it. But, for people that didn't experience that, it's historic.

RP: There were some parts of the ORL which were really a pit. The place where the counting was done down under the building — it was a pit. I don't remember exactly what it was but...There were some pretty crummy parts of it. But the labs. I worked in upstairs were really kind of fun. I got a little hot under that tin roof up there...

VM: Did you notice a change, then, in attitude among people when you moved down to LSB?

RP: I'd only been six months, I think, in ORL so it's a little hard for me to say. I think we were more isolated from each other. In other words, (*in LSB*) for me to walk to the main lab. I had to walk 100 feet or so and around two corners and down this wide corridor and open a door and go into other labs. That made it different.

SM: May I ask a question? I do remember. Was the planning for the new building, had it begun, and had the application for funding and so on begun, before the Nobel Prize excitement or after.

RP: I think the new building was in Calvin's mind and a lot of minds — it wasn't shared with me — because they knew that ORL was going to go, and that Crocker (*Lab.*) was going to go, and that they knew LSB was not permanent. Before I got there, there was already in everyone's mind the notion that he would get a new building. Now the Nobel Prize came along — I can't remember — about six months after I got there...no a year and a half.

VM: No much later than that; the Nobel Prize was '61

RP: '61? Well, I can't remember: two and a half years. But people sort of thought that was going to happen. One of the things that occurred, as long as we're on the Nobel Prize, was, as you know, there was this "competition", I guess is the best way to call it, between Arnon and Calvin. Roger Stanier thought he'd be the great mediator and he organised this photosynthesis seminar which was about a year or so after I got there. All of Stanier's people came, Arnon's people came, Calvin's people came who were working in photosynthesis. Stanier was sort of going to get these great minds together and have a synergy occur between something where one and one had been less than two; he (Stanier) was going to make it three or four and sort of bring all this creative energy together.

Well, they decided who would give the first seminar. It turned out to be me, to be I rather; I would be the first one. I pulled my material together, I looked out at the audience. I remember it was in the old Botany seminar room on the south side of LSB there on the first floor. I started and Calvin's people were on the one side and Arnon's people were on the other, and they (*Calvin and Arnon*) were sitting in the front row with their people behind them. We got into some sort of rate discussion about something I was doing, and Calvin and Arnon started at each other. Roger was sort of standing there with a blank(?) (*indecipherable*). Calvin was sort of saying "well, the problem was such and such" and then Arnon would say "well, I regard this very much as an athlete who is running and in training and the question is, does he have a large heart or a small heart?" He would start along with these analogies and Calvin was just beside himself. He said "to hell with the athletic hearts! Let's get back to photosynthesis!" Anyway, this thing was a disaster. There I was, standing up in front, this wet-behind-the-ears postdoc. with kind of these missiles flying in every direction, not at me, it had nothing to do with what I was saying. It was totally out of control and Stanier was trying to handle it. I remember those days well.

VM: So nothing happened? There was no reconciliation?

RP: (Great laughter!) There was no reconciliation at all. It was trains passing in the night!

VM: Do you know what the original antagonism between Calvin and Arnon was? Nobody seems to remember it or...

RP: I don't really know. Arnon used to say, "look, everything in the Calvin cycle occurs in animals also, the unique thing is photosynthetic phosphorylation". He obviously had the view that he should get the Nobel Prize because he was doing the only thing that was uniquely plant-like, which was cyclic phosphorylation, and if you took what Hill did, electron plant transport that was one, you added phosphorylation, but all this carbon cycle stuff occurred in animals. That was sort of his (*Arnon's*) argument. Of

course, he had started as someone in plant nutrition and had moved over and started in this area with Bob Whatley. I think it was direct competition for recognition, ego gratification.

VM: They were a couple of powerful characters.

RP: Very much so.

VM: Another thing in which you were almost the only one for this to happen, is that you had a faculty position during the time that you were a full member of the Calvin group. I think only one other person (*Ken Sauer*) was in that position.

RP: Dan Mazia and some others apparently went to the dean and said that they felt that there should be some positions opened up, since the place was expanding and new faculty, additional faculty being hired, for people they could just identify as targets of opportunity. I was asked by — I think it was Sandy Elberg (*Sanford S. Elberg*) and Dan Mazia and some others who were on this committee — if I would be interested in an appointment. They didn't even say exactly where it would be. I said I thought my background was such that it should be in Botany and Plant Physiology, though Chemistry was not out of the question if, in fact, that's what I wanted to do and the chemists went along with it. I should have thought, deep in my heart at that point, what I would be best at and I felt I would be best in a department that did everything from looking at fields of plants and doing ecology all the way down to detailed analyses of how certain reactions work. I would be happier there than I would in a chemistry department because my background was more like that.

So I decided to go with the Botany Department which was a bit strange in many people's minds. I think it was the right thing for me to do. This was apparently an initiative by Dan Mazia and Sandy Elberg and a few others to make a recommendation to the dean. What I got was half-time appointment and then it went full-time in '64.

VM: With the half-time, you were regular faculty, on a half-time basis.

RP: Yes, on a half-time basis, teaching and...That worked out pretty well because we were down in Life Science Building over that period. As they (*the Calvin group*) went back into the new building it was a little too much of a split to handle well so I went out and got some grants and told Calvin I wanted to go full-time in the Botany Department, which I think upset him a little bit. I went to see Henry Rapoport before I went to see Calvin and said "what do you think I should do? I sort of find myself trying to work on my own here and get a lab. going at the same time doing work in Calvin's lab. which is fine, but it's not satisfactory in terms of the totally focusing of my effort." Henry said "well" — the way Henry would — "it's pretty important to be able to call your own shots". So I thought about that a bit, that conversation, and I finally if I decided if I was going to do this right I would have to fly on my own and so decided to go full-time in the University .

VM: But you retained an association with Calvin's lab.

RP: Yeah, I did retain an association over time and could still use the equipment up there and help his graduate students and do other things. But I had my own lab. and my own grants, and so forth that I ran down in the Life Science Building. It turned out to be in exactly the same lab. that I had been in, on the first floor, that the algal lab. had been in.

- VM: You'd thought you had got out of that building!
- RP: Well, you know, it was kind of a crummy building. When they emptied the dishwasher upstairs all the foam and all the crap would start coming up through the floor and that kind of thing. It was awful. It was a terrible building. It's a good thing they gutted it and rebuilt it.
- VM: Coming back to the round building, do you think the effort was satisfactory? Do you think that they did, in fact, recreate the atmosphere of ORL as well as one could in a new building?
- RP: No, I don't think so. I think...perhaps I should go back to the comment that I made earlier that when buildings are complete and beautifully finished they become somewhat sterile. I never found the same kind of interaction that might have occurred. Part of it had to do with the fact that Mel Klein's group, for example, was downstairs again, through the closed doors, not via the spiral staircase. You had to go through another door, to make an intentional trip down there to talk to him. I think losing the physicists, which kind of got lost, detracted somewhat. I think the chemical part, the biological part on the second and third floors worked pretty well, but there was something about the segregation of the physical chemistry, physical side, that you didn't walk through and didn't see and didn't experience in the way that we used to in ORL.
- VM: They chose it like that, as I remember. They didn't want to be in big open labs. for their own reasons and that was one of the reasons why the ground floor was separately configured.
- RP: Thinking back on it, there should have been some more spiral staircases or something. There was something about that that in my view didn't work as well.
- VM: Was there also, do you feel, two other considerations. One of them was the size. It was obviously much bigger than ORL and more people, and then there was the bringing together of the Donner group which did not have the same intensity of focus as the ORL people had had.
- RP: That's true. The Donner people, there was — let's see — Dick Lemmon had his isotope bombardment (*equipment*); that was again behind a closed door down on the first floor. There was Ed Bennett with the brain biochemistry. But all of those didn't really get integrated in the sense that they were on that first floor. Actually to leave the second floor you went through a fire door at the top, a fire door at the bottom, down the hall and through another door to get to somebody. I think that was enough to make it more isolated.
- VM: There was also, I suppose, the fact that the photosynthesis activity became very different by the time the new building was open. There was no longer the focus on solving the carbon cycle and what immediately followed it. It began to spread into all sorts of areas.
- RP: There was Al Bassham doing his work and...
- VM: Your work was quite separate.

RP: ...quite separate. A lot of work on electron transport and structure and certain physical-chemical measurements and others. I think the techniques became themselves more isolated than everyone working on the segments of the carbon cycle. It was different. You can't really recapture things. Calvin's view was to have interdisciplinary synergy in that lab. and, as I say, I think it worked pretty well on the second and third floors but I think somehow the physical part on the first floor got removed by three doors. That was enough to make it separate.

VM: But I am sure you value the whole thing as being an interesting and pleasant time of your life.

RP: It was a very formative time and a remarkable opportunity to participate in a grand adventure and one is indelibly inscribed by those experiences and I think they were very positive for me, particularly the opportunity to talk to and work with the people — Mel Klein and others — on the physical side whose way of looking at things was as a physicist, which really is a different way. This was extraordinarily valuable to me. Ken Sauer is the same way, looking at things as a physical chemist and the kinds of questions he would ask were not the ones I necessarily would have asked, but influenced me because these were the kinds of questions I subsequently did ask.

It's one of the great difficulties in our disciplinary universities. As our fields became more and more specialised, one worked with a smaller and smaller select group of colleagues on more and more specialised areas and one didn't have this interaction. Calvin may have foreseen this over-specialisation that occurs. What has happened now is that people don't even work with their colleagues within the university. The work on the Internet with colleagues around the world who are specialised! We lose community and we lose the synergy that comes from trying to understand the broader dimensions of our science and our problems.

VM: How do you see him (*Calvin*) as a leader and an inspirer and an innovator?

RP: See whom?

VM: Melvin.

RP: Melvin...I saw sort of at the end of his carbon cycle career so I never saw him at the really creative part when he and Benson were initially doing this work and putting this excitement together. At the end of his career, I saw him more trying to regain the excitement and the prominence of what had occurred during those exciting days of the carbon cycle. He is very inventive and very thoughtful about a lot of things. Let's face it. You don't very often create one great kind of discovery after another. So I think, in my experience with him, I found him a bit more frustrated and slightly desperate to regain what had been than what he must have been when he was right on top of the wave. He slid off the wave, everybody does. You never stay on the crest of the wave forever. I didn't see him at probably at his creative peak.

If there is anything I would say, that is characteristic of him, is that he didn't have the psychological resource to enjoy his success and to be satisfied with his success. Insecurity followed him forever if he wasn't on the crest of the wave. He felt somewhat insecure. I suppose as you get older you get over that but if you get more experience you get over that. Maybe if you have his creative desire and ambition you never get over it. You've got the monkey on your back your whole life in terms of trying to regain that sense of prominence as the symposium speaker that everyone comes to hear who's made the latest great discovery. I saw him in the years when he

had drawn away from that or had gone beyond that. He was having, I guess the term to use is that he was having withdrawing symptoms.

VM: The other people in his lab., the permanent people: there were some like yourself and like Ken (*Sauer*) who moved to faculty positions either in Berkeley and then people like Andy (*Benson*) and I moved away for faculty positions. There were some people who stayed there. Do you think they were a good group of people, high quality people throughout? Had he been successful in gathering the right people around him?

RP: I think there's a fundamental problem: that really excellent people are not going to stay working for Calvin. What he would have to do to create a group was to find people who were very good but who could endure always being a little lower down in the organisation and not kind of their own boss. Now, you can get a lot of good people that way. One of the problems you run into is that they tend not to be as versatile and you tend to have more trouble picking up exciting new initiatives with those kinds of people because they have been selected around certain programmes. So in a way, the best people he could attract aren't going to stay. The people that do stay are never going to be necessarily the people you'd want. The best thing to have in those situations is what he had with his postdocs. and others coming in and turning over. That makes it by far the best way to do it. It's the same thing in any lab. When you hire a technician, at a rather low level, you'd rather hire a really young bright graduate student who is going on to other things, that you have to train and then they are gone after two years, than someone who's going to be there thirty years and be really dull and never learn anything new. That would an extreme example. So in a way, to keep a lab. going with a permanent staff almost guarantees that the place is going to become a little slower and less interesting over time.

VM: There is one saving grace which is worth reminding ourselves about. And that is that over the course of time, and certainly by the sixties, there were several faculty members who were pretty independent who were still associated: you, Ken, Rapoport, Tinoco, John Hearst. These were all people who were not totally beholden to Calvin.

RP: That's right. I think that was difficult for him but I think he recognised that and it did keep the place vital. Of course, one of John Hearst's students, Tom Cech, went on to become a Nobel Laureate himself with the RNA enzymes, and so forth. There certainly was a continuity of excellence there. And Calvin himself? I don't know what his health has been over time..

VM: Unfortunately now he has deteriorated quite badly.

RP: I think when his wife died that had a rather severe impact on him. She had done so much for him. I had always thought that she was keeping him alive and then suddenly she was gone.

VM: Well, she had been healthy but she incurred a disease which is unpredictable.

RP: I guess we are down to discussing what Melvin Calvin is like. He has certainly been a creative kind of a giant in the field and a remarkably clear thinker, perceptive. I wouldn't say his strongest characteristic was leadership the way I think of leadership. I think when he was actively engaged in something really exciting people wanted to join him and work on it. I can't exactly imagine him as captain of a ship, successfully, if you put leadership in those terms. It's more a ...

VM: It's the field marshal, not the general. He's the guy who sets objectives and gives directions. He doesn't order the troops around.

RP: That's right.

VM: Just a few specific points before we wind up.

SM: My interest generally is people, the kind of thing I make notes on. I was wondering how — you talked about your settling down to work with Melvin to start with and your early reaction to him. How do you feel that he got along with other people? Do you think he was an encouraging sort of person or was he harsh; how to think about that?

RP: I didn't see a lot of his personal interactions with the people in his office. What I saw were the interactions at the Friday morning seminar, perhaps was the...I didn't see him go around the lab. too much. Once in a while he would come around and talk to people. I wasn't there when he was talking to them. What I am saying is coming primarily from the Friday morning seminars. He was very interested in what people were doing, he was extraordinarily — and maybe rightfully — impatient with people who could not present their material in a logical, understandable way, and he would get quite angry and lose his temper. That for some people was not traumatic; for some it was extraordinarily traumatic. A number, I think particularly the graduate students, were afraid of him; some of the graduate students were.

On the other hand, if you knew your material and knew what you were trying to do, knew what you had shown and hadn't shown, and could anticipate the weakness in your argument and say "so far we don't know this, it's something we have to do, let me show what we have done", Calvin was terrific. He would go right along and he would contribute and have all sorts of ideas and he would very often see relationships with other kinds of science that weren't so obvious. I think he had, and this is not an unusual characteristic, a lot of trouble — and this was not conscious, this was unconscious — dealing with people that he regarded as not very smart, or not as smart as he was. Let's put it that way, which meant that you had to have pretty high standards! His interaction was very good with people that he felt comfortable with scientifically. On the other hand, with people who should have known better, he was merciless. With the graduate students, he realised that they weren't that experienced, but I think sometimes he was not exactly parental in his guidance and encouragement. I don't see him turning a C person into a B+ person very well. I can see him turning a B person into a D person pretty rapidly! He was a very intelligent, quick, demanding taskmaster who didn't suffer fools gladly. He could be very impatient and that "Hell with the athletic hearts. Let's get back to photosynthesis!" ; that's exactly the way he treated Arnon.

(Tape turned over. Opening question apparently missed)

SM: ...That's important too; what do you remember?

RP: About the social life in the lab.?

SM: Well, the very first Christmas I was there I was Santa Claus at the Christmas party, I remember that. I remember a party up at the Calvin ranch up in...where was it?

VM: Healdsville.

RP: Healdsburg and having a lot of fun at that. I remember some parties at his house, which were graciously and beautifully done, we went to. I did not have a real social life with the people of the lab. outside the lab. as such. I mean, we'd have people to dinner on occasion but social life got built around other friends, some of whom had nothing to do with the University. I got to know the Evans family, Evan C. Evans III and his family, and various other groups, and those were the people my wife liked.

VM: You were already married when you joined the group?

RP: Yes.

VM: So that in a sense separates you from the transient postdoc. and students...

RP: Somewhat; that's right.

VM: And, as I remember, you also lived across the hill, in Orinda?

RP: Yes, we bought a house in Orinda because it was cheaper. I could even tell you how much it was: it was \$17,500 with a \$14,000 GI loan on it and 4-1/4% or something like that. I mean it was so cheap in those days: £135 a month paid for everything.

VM: But \$135 a month was more, of course, then than it is now.

RP: It was more then; true. So we lived...that was also true; we didn't live there. So my social life was not built around the people in the Calvin lab.

VM: Did that mean living in Orinda you were really a bit too far to pop back in in the evening after dinner, or not?

RP: No, I used to come back or stay in. I worked long hours. I worked on Saturdays. It wasn't too far, it was about 20 minutes or something. There was less traffic in those days. I got used to that. It was better to work long days, get there earlier, leave late, see the children in the evening, than come back.

VM: The last thing is: what happened to you post-Calvin, post Calvin lab.?

RP: Post-Calvin I went on and I worked in my own lab. down in the Life Science Building. I probably had one of my best scientific years about 1970 when we separated the grana and stroma lamellae out of chloroplasts and showed that one had only system I and the other had system I and II, and came up with some interesting models for how one could explain certain photosynthetic phenomena on that basis. That was with David Goodchild and Raj Sani that year. We published a review that was one of the most widely cited papers and reviews that year. It was also a year ('69/'70) in which we had riots on the campus and people marching through, painting the walls and spray painting and tear gas coming in through the ducts and people coming around telling us at one o'clock the building was going to be bombed and we had to get out. We never went out because we had spent all morning, since eight o'clock, making some biochemical preparation that we were going to use and we weren't about to leave it. And no bomb ever went off.

VM: This was Life Science Building?

RP: Yes. So actually, that again, was the fact that some of the best work is done in the crummier buildings under crummier circumstances, was another example of that. I

went on and became, at that point, a chair of a new department, which was the Department of Instruction in Biology, and did a lot of work in setting up the new curriculum, new biology labs., working with Dave Hackett and Gunther Stent and a lot of really fine people — Oscar Paris and Dan Branton and others — setting up these courses.

VM: By this time, of course, you were a full professor?

RP: I had become a full professor by then. I was a Miller Professor in 1970. In 1972 I was asked to become the Dean of the College of Letters and Science. I thought long and hard about that and I realised that we had some 900 faculty members in the College and that only four, or three or four, would ever be at the right stage in their career, the right time, to become a Dean and maybe I should taste this dish in the buffet of life. I wasn't sure I could handle it. Everything I have taken like that I wasn't sure I could handle it, that's the reason it became interesting. So I took that over; we were recovering from Cambodia, getting the curriculum back in shape. I did that for probably too long, for eight years. I probably should have quit after six or seven; I think that's too long to be a Dean.

At that point my marriage was falling apart so I wanted to find out whether I was OK or not. So I went on a single-handed sailboat race out to Hawaii and back. By the time I hit the dock, I felt I could do anything. I never felt so strong in my life; just an amazing experience. It turned out that the trip, during the time over there and the time back, was one hour short of forty days so it had a biblical roundness to it. A very important experience in my life in terms of knowing what I was made out of. I was stronger than I ever realised. We all need social support. The question is whether we need it all the time or can we get along some period without it.

So, I got back. I found out the search had been going on to replace Mike Heyman who had become the Chancellor of the campus, replacing Albert Bowker. So they needed a new Vice Chancellor, which is sort of like the Provost here, the executive Vice Chancellor. George Pimentel and I were the finalists in that. Heyman interviewed both of us and then he selected me rather than George. Then I spent the next ten years as the Vice Chancellor, working with Mike as sort of Mr. Inside and he was Mr. Outside, in terms of what we did. I was sort of the architect behind the total biology reorganisation and the construction of the new buildings.

SM: This was around 1980?

RP: 1980 through '90. But the buildings were not finally completed until '92 or '93. But the project had started when I was Dean of Letters and Science, and Fred Carpenter was the chair of the Division of Biology, or the Divisional Dean for Biology. We started an inventory of all the faculty members who did biology on the campus, regardless of departmental orientation, and what they did and grouped them by what they did to find out what we were good at and not so good at. And then went on with this for the reorganisation and built the buildings around the reorganisation. That was a 200 million dollar project and a lot of fund-raising. That was my major contribution, I think, over those years. There were a lot of other things we did but that's the one that one sees, left behind as a kind of a monument.

VM: What happened to your science during this period?

RP: I published all through the period with a man named Fernando Henrique. Not so much and my very last paper came out just a year ago this month (*i.e. in July 1995*),

which was starting to do some molecular biology and sequencing of genes. This was just to learn how to do it with ubiquitin in conifers (in *Pinus sebiniana*) which is different from other ubiquitin. It has a kind of very highly conserved gene, I know, but we published some things.

I was going to go on with this but then the University offered me this opportunity to make a lot more money in retirement than I would as a professor and I decided I would go on and do other things. I had started working with ConAgra on a potato project in Russia and I spent some time on a farm outside of Moscow where they were thinking of doing a potato seed operation and learning a little bit. We planted, along with a fellow from ConAgra, about 8 tons of the first several varieties of the first US seed potatoes ever to go into Russia.

SM: How long were you there?

RP: I was there about three weeks, three or four weeks. We had a hell of a time, getting everything to work, getting the potatoes there without being stolen, just one thing after another. This was in Krasnygorsk which is north-west of Moscow. To give you some idea of how it worked: the collective farms were getting about 8 metric tons/hectare; the privatised farm we were on, which had about ten farmers, was getting about 10 metric tons/hectare. Our seed got about 30 metric tons/hectare because theirs (*i.e. their seeds*) were so badly virused. What they did every year was to take the smallest, scaliest potatoes and use them as the seed for next year — a good Lysenko approach! (*Laughter*)

Anyway, of course, the farmers were interesting; they were highly educated in mathematics and various other things but they absolutely had no idea of investment. You could not even ask the question for the interpreter “how did you finance your crop?” It wouldn’t mean anything. Or “how did you sell your crop?” The stuff was just given to them, somebody else came and took it away and they might be given a tractor at some point. It was totally different. I found out one interesting thing. I took a Powerbook with me. They were extraordinarily interested and good at gambling. Because they had these casino card games in the (*Power*)book that could be played and they were really interested in that. Obviously, you could see how to teach them. You could start with gambling and about investment and odds. This was certainly a way to do it.

And then I was asked to be a consultant with all this blow-up at CU (*University of Colorado*) Boulder. Joe Cerny put me onto that. Apparently he was called by his colleague Chris Zaffartis who was the Vice-President for Academic Affairs, worked in the President’s office; they both were nuclear chemists and recommended my name. I went back there and worked in the spring of ’94 with two other people, Joe Kaufman, who had been the System Vice-President for Administration at (*the University of*) Wisconsin and John Schaeffer who had been a very successful President of the University of Arizona.

We came up with a really tough report about what was wrong on the campus, what was wrong in the President’s office, what was wrong with the regents about the fact that they had a democratic tyranny and the structure didn’t make any difference, everybody ran to everyone else and did end runs. It was really just a mess, a disaster. So, we said a lot of tough things. I remember when the plane took off that May from Colorado, having the same sense of relief when a plane took off from Russia. You know: “Who the hell is going to fix that mess?” About a month later I got a call saying “well we didn’t get our chancellor (Tim Mosely from Oregon), would you be

interested?" I said "let me think about it". I was very surprised to get the call because I had been so negative, not insulting but very critical of what people had been doing. I thought about it for a while and I thought "well, gee, it sounds so difficult, it's sort of interesting; that might take everything that I ever learned and every bit of intuition I had to begin to straighten that one out and you really can't fail, because it's a failure already!. And so it might be interesting to try that for a while." So...and I said I would do it no more than two years. Well, the two years is up and they wanted me to stay longer and so I'm in for another six months, but I really want to get out. That's what I'm doing at the present time.

VM: Thank you very much. That is a most illuminating discussion and it will go into the archives. Thank you.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

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Organizations in which you are active University of Colorado

Chapter 26

ANTHONE (TONI) L. PHIPPS

Berkeley, California

July 9th, 1996

VM = Vivian Moses; TP = Toni Phipps; SM = Sheila Moses

VM: This is talking to Toni Phipps in Berkeley on July 9th, 1996.

Toni, how did you get to Calvin's lab.?

TP: We had recently married and I was looking for work. So I went to the state employment (*office*) and they told me about the job (*at the Rad. Lab.*) I went up there and I was interviewed by Bert Tolbert. They accepted me; I went to work practically right away and started in with working with the glassware, not having a degree in physics or chemistry. I did this and then finally the job evolved to where I was in charge of the stock, the chemical and all the physical (*indecipherable*), ordering from The Hill — I did that and it developed into quite a big job.

VM: So let's take it stage by stage. If it was when you first got married, that would have been in '49 or '50?

TP: Yes.

VM: Where did you work?

TP: I worked in Donner.

VM: Was it in a big lab. or did you have your own room where you worked?

TP: No. We had one room at the back end of the long hall was our department and then to the left, about halfway down the hall, was Bert Tolbert's office and our secretaries and all were there. And then, as we got further down, there were different ones and members of the group (*indecipherable*) who had grad. students and all working with them. And then the big lab. was on the right hand side at the end of the building.

VM: When you were in Donner you took care of all the glassware did you?

TP: Yes, I did. There was nobody else. There was a second department, if I remember correctly, over in...

VM: In ORL, wasn't it?

TP: ...in ORL and there was little building over there, behind ORL, that they eventually...

VM: (No; that's fine...nothing's happened)

TP: ...tore down.

VM: But ORL was that old wooden building — you remember?

TP: Yes.

VM: ...and Calvin had people in there.

TP: Oh and...I can't think of his name now...he's now in southern California.

VM: Andy Benson.

TP: Andy Benson. Yes; Andy Benson was in charge there.

VM: Did you see the people over there very often?

TP: In ORL? Not very often. Of course, they came and went but I didn't see them very much.

VM: So you didn't know who they all were?

TP: I knew who they all were. When any thing, any group meeting (*was held*), or Dr. Calvin called a group, well we would all be there together, so this way I knew they were distinguished as being in ORL.

VM: What sort of social things did you do together, in those early days when you were still in Donner and the other people were in ORL? Did you get together out of hours, did you have parties?

TP: We had parties. Of course, we always had the coffee breaks — they didn't participate every Wednesday, I think was our coffee break day, “goodie day” as we called it.

VM: And everybody came?

TP: And all the building...if anybody had an occasion to be in the building, I mean those from ORL, if they had an occasion to be in the building at coffee time, why they joined in and became a part of the group just like the others.

VM: Did you celebrate things like birthdays and engagements and weddings and things like that?

TP: No.

VM: Christmas?

TP: We had a Christmas party, always had the Christmas party.

VM: That was both groups together doing that, was it?

TP: Yes. The whole group would be in. And, of course, we are still talking about Donner.

VM: Yes.

TP: Where did we meet in Donner for the Christmas party?

VM: I was about to ask you that?

TP: That was before your time.

VM: Well, if you went there in 1950, yes that was before my time.

TP: I can't recall now where we had our Christmas get-togethers. (*Editorial note: The Christmas party, starting in 1948, was held in the big lab. in ORL for the people from Donner and the people from ORL.*)

VM: When we had our first Christmas in the group, which was in 1956, it was in ORL. It was, in fact, around the big white table and I think all the presents were piled up on the table where all the food was.

TP: Oh, yes. I guess I had forgotten that.

VM: So you would come over then, all the Donner people would come over then?

TP: Yes, yes. We'd all get together, wherever it was. There was a group down in LSB.

VM: I think that came later. When they tore ORL down, they moved them down to LSB.

TP: Yeah; maybe so.

VM: You never worked in LSB, did you?

TP: No. I never worked in ORL either.

VM: Only in Donner?

TP: Only in Donner and in the new building.

VM: When they moved into the new building, which I think was toward the end of '63...

TP: Was it?

VM: Yes...you came over then and everybody joined together.

TP: Yes.

VM: And what did you do in the new building?

TP: In the new building, they built this as Dr. Calvin or someone told me — it was Dick Lemmon — you are going to have so many thousand (*square*) feet of storeroom. So then they built that room with the idea of it being a stockroom and they put me in charge of that.

VM: As soon as you moved into the new building, that's what you started doing.

TP: I moved in and I was taking charge of that.

VM: Very different for you to do that? Had you any experience of doing that sort of thing?

TP: No. I remember one Sunday, Ed Bennett was in the building working and he came down and helped me sort of sort out my stock and get things arranged. This was a Sunday afternoon; my husband was helping me too, to get this organised, because I was sort of overwhelmed with all of this stuff coming from ORL and coming from LSB and Donner. We had stuff stacked all down the hallways.

VM: What was the position about stockrooms before then? In Donner, was there a stockroom?

TP: Yes.

VM: Who ran it?

TP: Nobody ran it, really, as I know. It was there, of course, when I came; it was already set up, and if somebody needed something, you could call The Hill. The main stock supply was on The Hill. You'd phone up there and get whatever you wanted. If you had to order a case, well then what you used and then the rest would go in to this stockroom.

VM: There wasn't a stockroom keeper who kept the stock up on a daily basis?

TP: No.

VM: So When you went down and began to work in the new stockroom in the round building, did you have any catalogues of what there was, any lists or did you have to go around and sort everything out and find out what there was.

TP: I used my own initiative to go ahead and organise this (the stockroom) and put all this type of flasks here — this would be the flask shelves, I designated — they left that up to me, practically. I organised it as it turned out to be pretty efficient. And then I'd label; I had made...What was the guy's name who was in charge of the building?

VM: Paul Hayes?

TP: Paul Hayes. Paul: I would ask him for certain things and get his OK sometimes. I know about labelling on the shelves, as to what this shelf contained certain sorts of thing; you know, like. It was kind of fun because I kind of like things in their place, you know, and I like things in order. So that was fun for me to do and I enjoyed it because I got it straightened out after all those cases of stuff that were lined down the halls, to finally get (*organised*).

I know one thing which was quite funny. Dr. Calvin had a little box of vials, of very special chemicals that he had — I don't know whether he had developed this thing, but they were special, a special little box, it had about six little vials in it. It was missing. Paul said "oh dear; we got to find this. This is Calvin's and he will raise hell if we don't find it." I said that I haven't seen anything like this. Well, the whole thing...specially Paul and myself because we were directly responsible for it. Finally we found it and in the hall back over...the names have escaped me; you know, where

the seminar room (was) on this hall. You could walk through the stockroom and go out a door over into the hall on that side, which was on the hill side of the building. Well, when they delivered all this stuff from LSB, as I said, it was lined all down the wall in the hall. Well, one of those big, heavy doors...this little box, which was about the size of say 8 x 10, little box was...this door had swung back against it. They had been cleaned out...everything was all straight and in order and this little box was hiding behind the door. What a relief! Paul especially; he was really relieved to find the box. He took it in and put it in a vault in his office.

VM: He had a vault in his office, did he?

TP: He had a little vault...well, you call it a...

VM: A safe?

TP: A safe; that's what I mean, a safe.

VM: The stuff wasn't broken, was it?

TP: No, everything was in order.

VM: So when people wanted stuff out of the stockroom, did they come and get it through you?

TP: Yes.

VM: They just didn't come and take it themselves?

TP: If they knew what they wanted. Some of the grad. students I could help in the assembly of extraction (*apparatus*): I knew some few things like this, and help them get the equipment to set up an extraction or a distillation equipment or something. That's the way it worked. And if they wanted: "I need such and such and such a thing and order it for me" and then I'd order it from The Hill and then I would notify them, phone them and tell them that it's in.

VM: What happened if The Hill didn't have it, if you had to go outside to order it?

TP: No, I didn't go beyond that. The real heavy, expensive stuff, like pieces of equipment, was done through Paul. I would tell them, direct them to Paul if they needed something special because he was authorised to do that. I wouldn't know anything about handling that. I just handled everyday organic and inorganic chemicals and glassware and everyday equipment.

VM: When people took stuff out of the stockroom, did they tell you what they had taken so you knew to reorder it?

TP: No, not necessarily. But I kept track of my stock and if I see these beakers were getting low or something like this, I didn't have any system of, any records or anything that I could keep, only what I established myself.

VM: Even among the chemicals, you didn't know what was there.

TP: Yes. I knew about the chemicals. How did I know about the chemicals? Well, I knew what chemicals I had and if I had more than one of a certain kind, I would see that

maybe someone...You see, that stockroom was open and grad. students and everybody 24 hours a day. The students coming in there working all night. One incident that happened: we found that there was a student who had a mania for stealing things and I had a big key ring with keys to the closet that had the...what were they? They were caustic chemicals and things like this.

VM: Dangerous chemicals.

TP: Yes, dangerous, because it was in this little fireproof room. He stole the key and came in and took things out of the lab.

VM: Did you know who it was?

TP: They finally found him with the keys. He was a student. I guess he was kind of familiar with the work we were doing. But they got him; they arrested him for this.

VM: Was he a student in the lab. or from outside?

TP: He was from outside. He didn't work there. But, you see, then they began to be careful about who could...They would come in there, those students would bring their friends in and they'd use up all the coffee, they'd use up all the cream and sugar and things like this at night. You would get there and go to make some coffee or something and there was just a little bit of thing where they had been drinking it all. They would bring their poor, hungry student friends in there and fill them up!

VM: I remember one time, but I don't know whether you were still in the stockroom or whether your successor, who was Bill McAllister, was there. I remember at one time we tried to make a catalogue of everything in that stockroom so we could keep track. It was an enormous business and it didn't work. We made a list of all the chemicals and I think we put them on cards so that people could see...if anybody wanted a chemical you could see whether we actually had it without going through all the bottles. But within a few weeks, I think, it had all gone to pot because people took stuff and they didn't write anything on the cards. So we never knew — it just got into the same mess as it was before.

TP: Of course, I was there, my desk there and I'd see pretty well. One time somebody up on the third floor, I can see her now, was having a Christmas party and we had this alcohol in the big carboys with the syphon and all on it, and they were coming down there every now and then, somebody would come and go in there and draw out a beaker of alcohol. And then pretty soon, somebody else would be coming down and so they were supplying this alcohol for their Christmas party. So I told Paul about it and, of course, they put a stop to that right now.

VM: I think that's somewhat illegal, isn't it.. Apart from stealing, you're not supposed to drink that alcohol.

So you were there as storeroom keeper until you retired from the lab.?

TP: Yes.

VM: When was that?

TP: In '70?

VM: I don't know. It was before I went because Bill McAllister was already there for at least a year or two.

TP: He was the one I contacted on The Hill whenever I ordered anything. He would generally be on the phone and would send it down to me.

VM: So when you left, he came down from The Hill to replace you?

TP: I didn't know that.

VM: You didn't know he came down?

TP: I heard afterwards that he came down because he knew, pretty much...he was in the big stockroom on The Hill and it was through him that I'd fill my orders to keep my stock up.

VM: Your desk was in that stockroom...

TP: Absolutely.

VM: ...which didn't have any windows?

TP: No windows.

VM: Trouble you?

TP: No.

VM: Of course, you could walk out whenever you wanted to see the sunshine, couldn't you?

TP: Yes. I could walk out. And, of course, the elevator was right outside my door. And I also had...like I would get a big case of beakers, for instance, Erlenmeyer or some of those ordinary flasks — big boxes of stuff I'd put it up on the roof in the room up there and I could refill my stock shelves from there until I ran out and then I could another big whole case of the stuff.

VM: There was all that storage space under the sloping tiles on the roof, wasn't there, which people used for that and for spare books. What sort of memories do you have of working among...because you must have worked there for nearly twenty years, I should think.

TP: I worked about fifteen, sixteen years.

VM: You must have retired in about '66, '67.

TP: Around '70; I sometimes think, sometimes I wonder whether this is Alzheimers!

VM: I don't think you've got Alzheimers!

TP: I forget things that I'm not familiar...I put all that beside me years ago. It's like trying to pluck something out of the cobwebs and it's not very easy to do.

VM: I know.

TP: (Let me fix you a little glass of wine.)

VM: What sort of memories do you have of the people you worked with there?

TP: I had very pleasant...I had no unpleasantness, only with a gal who wasn't any good, unpleasant. But she was kind of a (*nut?*). Did you remember Altha Vann?

VM: No, I have heard the name but I never met her.

TP: She was a pain. She was a black girl but she would come in and she'd say to me, she was kind of eerie, "Oh, Toni, would you like to go to the (*indecipherable*); they are giving a formal dance in San Francisco, would you like an invitation?" I said "No, I wouldn't." Meantime, she is saying this at the door up there and broadcasting it all over the lab., which embarrassed me. So I' "Well, I can get you..." I said "I don't care to go. I have already declined the invitation so Bill and I..." "Well, I just thought it'd..." I would just go and ignore her but that was the only unpleasant thing. The personnel in my group, that I worked with, was very good. I had good relationships with all of them. I was invited to all the parties.

VM: And your husband went with you to parties?

TP: Yeah. They invited Bill: "Toni, you and Bill come now, be sure".

VM: The more you talk about him, the more there is some memory coming back to me of what he was like. Before I leave, I'll have to look at a picture of him and remind myself of what he looked like. Did you talk to Calvin?

TP: Oh, yes. Oh Dr. Calvin: he made a suit for Dr. Calvin.

VM: Bill did?

TP: He was a tailor for the officers on Treasure Island...and that little bear there, with the green candle in it, Dr. Calvin...he used to call her "Babe" you know; he'd say "Hey Babe, "isn't this cute?" When he'd come back (*for a fitting*) he'd catch my eye and he would look as if he would swipe it.

VM: I'd better explain for anybody listening that "Babe" was what he called his wife, wasn't it?

TP: He'd used to call her: "look, Babe,". We always thought Dr. Calvin would be so dignified, he wouldn't say...but, of course, he was just like anybody else.

VM: How did you get along with all the other people — Dick and Ed and Al and Andy and all those people?

TP: I got along all right with them, Ed and...I didn't have any unpleasant incidents to record at all because things went very well for me.

SM: They were a very nice group of people, weren't they?

TP: They were. To have a number of people of different personalities and all like this and doing some very technical work sometimes, they were all very good people as far as being congenial and all.

VM: You met lots of overseas students and postdocs.?

TP: Yes. That was the interesting part about it. I could interpret some of this badly broken English that some of them were speaking. I know there was an East Indian fellow and he had done thing there, you know, his studies and he wanted to go back home and he wanted to take a lot of baby food with him.

VM: Why did he want baby food?

TP: He'd gotten this little baby...

VM: I see; with him in...?

TP: ...and he can't get this kind of food in India that he was using over here so he wanted to take loads of this back. He wanted a wooden box. I said, "well that would be hard to be". We usually had cardboard which is just as firm and substantial as wood. He said "but I don't want this to get broken; I want wood". I said "well the only thing it would have to be built", so I directed him to The Hill and talk to stock up there and maybe they could get somebody to build a box. But he was very unhappy because I wouldn't order him a box to pack his baby food in. You know; all these kinds of things.

VM: But mostly they were OK and they didn't need a lot of baby food.

TP: No. He left, I remember that. Cyril Ponnampereuma...

VM: Was he the man?

TP: No, it wasn't he. He's down the peninsula isn't he?

VM: No, no. Well, he was. He left here and went down to Ames Lab. on the peninsula and then he moved to the University of Maryland. He died, three years ago, four years ago. I think he had a heart attack. He wasn't very old; he must have been late sixties, I would think.

TP: I lent him an ottoman because he was having a party and I was having a big ottoman, white plastic thing; I used to have it here. And he wanted something like...they wanted an extra chair. And so I said "well, I can give you card table chairs". And so he said "well no, he didn't want anything like that and he saw this old business and asked what (*indecipherable*). So he took it and used it for his party. There were a lot of personal things that happened, that I can't quite recall now. Some things were kind of funny.

VM: I think you have told us a lot of interesting memories.

TP: We used to go to parties all over the place. Everybody was giving parties. There would always be...and the rest of the gals...there were two other gals, black gals, in the building in Donner and they were jealous of me because I didn't have any reason to associate with them because my group was doing altogether different work and so we had nothing much in common. I didn't know them socially outside of the lab. and didn't care to, necessarily.

VM: One of them was Altha Vann, was it?

TP: Altha Vann worked in ORL.

VM: Who was the other one? You said there were two girls.

TP: There was one girl that worked...what was her name? Do you remember when she got stuck on the elevator?

VM: No, I can't remember that. Was it Alice Smith?

TP: Alice, that's who it was. Alice got stuck on the elevator. She was one of these kind of gals that has all these Sunday school Bible cards and when she wasn't busy she was sitting up there reading the Bible. I think it was so ridiculous. People that do this make me sick because they are generally the worst hypocrites in the world, you know, and they are always talking about this business. Anyway, she got stuck in the elevator and she alarmed the whole building. Because, you know, when you bang on the (elevator doors) (*indecipherable*), everybody was coming "hey, what's wrong?" She was in there saying "Oh Lord have mercy; Lord have mercy; Oh Jesus, oh."

SM: And did He?

TP: He did.

SM: She might have had it right!

TP: Paul came around and said "Alice, just stay calm, there's nothing that can happen to you. We've called the elevator service emergency..." (*Rendering of Alice Smith yelling and moaning.*) This was right outside my office door, see, and I came up there and (*told*) Alice "why don't you stop all this? Nothing can happen to you." (*Further rendering of yelling and moaning.*) Finally they came and released her.

VM: It took her a long time to recover, did it?

TP: Yes. She was fit to go so they let her go home after that. That was funny.

VM: There were lots of fun times there, weren't there?

TP: I know you remember...lived in Walnut Creek, lived in Pleasant Valley.

VM: Martha Kirk.

TP: Martha Kirk.

SM: She was a lovely lady.

TP: Martha. Martha; she would come down there sometimes and eat her lunch with me. Somebody would come in there with the broken English and (*she*) said "what did they say?" I don't know how I was able to decipher whatever they were saying. Martha would say "how did you know?" I would just get a gleam of what they were talking about.

(*What follows is no more than marginally relevant to this history.*)

And Bill Hart (*a laboratory glassblower*) made this for me (*referring to an ornament*). He had this large tubing and then he made the smaller diameter inside and then he told me to go to the hobby store and get some marbles, whatever colour combination that I wanted, so I got the brown and the blue, the amber and green and blue. He made this and, of course, I said "how does it go in diagonal lines like this?" He said, "'course, if they were square, they would stack and would be straight stacks. By being round, and rolling and falling in place like that." Somebody said they saw something like this in some store somewhere and it was around about \$69.50 or somewhere.

VM: Since people who listen to this tape can't see it, I should say that it's a cylinder about 10 or 12 inches high and 4 or 5 inches across and it has an annulus down the middle and between the two cylinders are packed coloured marbles, packed in such a way that there are lines of colours rising diagonally through the cylinder.

TP: It didn't originally have this core. Bill and I camped and fished a lot and I would pick up grasses and different things, and I saw this beautiful white, snowy-looking weed, it was nothing but a weed but it was really pretty, on the banks of some river up north and so I picked some of it and came back and put it down in between here. It was brittle and dried, and as it dried it fell off it just messed up all of the marbles. So I took it back to the lab. and I said "Bill, what am I going to do with this?" He emptied it out in one of those big developing pans and washed it all and put it back and made this lid...

VM: That's also glass, is it?

TP: Yes. This goes down in there, so I can still set it and I can set in in water if I want to make a centrepiece for my table or something. I can set it in water and put flowers in there and it still wouldn't get into the marbles.

VM: He was a very good glassblower.

TP: He was really an artist. He invited Jo Onffroy (*one of the group secretaries*) and I down to spend the weekend with him...

VM: ...in his...

TP: ...cabin down in the foothills near Yosemite. We went down there. He's a vegetarian so we brought one of those little bitty tins of ham, you know: Jo said "I like to have some kind of meat". So we bought this can of ham and all.

And anyway, he took us all around and he had this huge swing with these...what do you call ropes that are all stretchy?

VM: Elastic type ropes?

TP: No. These were yellow; anyway, they have elasticity like this and this was what this swing was made of. He had a tall, a big high limb where he had this thing. He said "Get in, Toni, and I'll push you". It swung out over a kind of a little stream he had there and so you swung out and oh, you got a long swing; it was really firm. Jo said "I'm waiting because I haven't done this since I was a kid." She got in there and she said "you know, when I was a kid, I used to go way up there, I used to jump out off of the swing". I had no idea that she was going to do this, at her age. After she swung

a few times, when she got way out over there, she jumped out. She wasn't any good for the next two days.

VM: I can imagine.

TP: It was funny. I didn't want to laugh, because she could really have been seriously hurt, but I had to laugh anyway because it was so funny. I had no idea she would do that.

SM: What was the name of the glassblower?

VM: Bill Hart. They were fun days, weren't they?.

TP: I enjoyed it. I missed you guys when I got home. I thought "gee whiz". You know when you work with...I spent the better of my waking hours with this group. I'd get up in the morning, throw down some cereal, go on to work and come home in the evening and take off my hat and coat, grab a skillet and cook dinner, and pretty soon — you read or something and the television or what have you — then you go to bed and get up and do that five days a week for years.

VM: That was the case for all of us. Many of us used to work at nights and the weekends as well, especially, I think, when we were younger. When we got older and had families, we didn't do it so much.

TP: Where are you living now?

VM: We are living in London.

TP: Oh, you're living in London.

VM: Yes.

TP: Oh, I wondered. Since you called me and all, I wondered where you set up if you're practising or if you're working or what.

VM: I'm retired.

TP: You've retired from the lab.?

VM: I'm retired altogether.

TP: I imagine you wouldn't be coming over here with a mission like this unless you were retired.

VM: I retired about three years ago. I'd better stop this...

SM: Can I ask Toni something. What have you done since you retired, Toni?

TP: I haven't really done anything that I can say I'm ashamed to say that. But I haven't done anything that's really constructive necessarily. I belong to a sorority and we did some little charitable things and little things like that. I haven't really done anything. I'm ashamed to say it but I haven't done anything really.

VM: I don't think you need be ashamed to say it.

TP: I am, because it is just wasted time. You don't pass this way but once.

SM: Did you enjoy it?

TP: I enjoyed it. Bill and I had...I had a very beautiful relationship with my husband if I must say it. We were compatible, very compatible. We liked the same things. I'm from the Middle West (Iowa and Nebraska) and what I knew about mountains and fishing, and like that., my father was a physician/surgeon and there was not outdoor life like that in our family. I thought the height of boredom would be to sit on a stream someplace and watch for a cork to bobble with a fish on the end of the line. He bought me some fresh water tackle and took me up behind the Claremont Hotel (*in Berkeley*) on Old Tunnel Road up there and taught me how to cast.

Then I got interested in it and he got me freshwater and saltwater equipment and I had my waders, and all like this, and we used to go all up in Northern California, camping — we had everything to camp with — and we had a ball. I liked it; he like to do the same pursuits. Few men like to shop; he liked to shop.

VM: Shop for groceries?

TP: Groceries. I was telling a friend: we were buying melons the other day and I said whenever I buy honeydew melons I always think about Bill. I'd be standing there; he and were shopping and I'd go over to the melon counter and I'd be looking at melons and he would say "stand back, stand back there, let an expert get up here". He would pick out this melon and when we get it home it would taste like a pumpkin. And he laughed about that; we would have a little laugh. We had lots of fun together. He liked to work in the house. He remodelled it: see, he knocked all this out here.

VM: I think we are going to have to draw this to a close because the tape is about to run out. No, no, don't move.

TP: I must fix some wine.

VM: Before you do, I just wanted to thank you very much for all that you have told us. It's nice seeing you again after so many years and nice to hear from you. Your record will go down to posterity on this tape.

TP: I'm flattered, really — I'm really flattered to think that you put me in for posterity.

SM: I'm glad we were able to get to you because we had some difficulty between us in fixing a date.

University of California
Berkeley, California 94720

(Please write clearly. Use black ink.)

Organizations in which you are active

Chapter 27

BERT M. TOLBERT

Boulder, Colorado

July 17th, 1996

VM = Vivian Moses; BM = Bert Tolbert; SM = Sheila Moses

BT: Today is July 17th, 1996 and we are in Boulder, Colorado at the home of Bert and Anne Tolbert at 444 Kalmia Avenue.

VM: Bert, can I start by asking what your early scientific history was and how did it happen that you eventually joined Calvin's group?

BT: Yes. I went to Berkeley after two years of a junior college in Idaho and took my bachelor's degree there in chemistry, completing it in 1942. Following that, I worked a summer at Standard Oil of California and then I went back to the University to do graduate work in chemistry. After approximately a year of graduate work the pressures of the war had increased and we were moved from our teaching positions in the Department to join the Manhattan District working on uranium chemistry.

VM: That was still in Berkeley?

BT: Yes, that was still in Berkeley. So I joined the group there and we did research and work in uranium chemistry. I even worked part of the time on a temporary basis setting up equipment down in Oak Ridge, Tennessee. That work continued very strongly, of course, until the atomic bomb was completely developed and was exploded. Then the war came to an end. At that time, then, we did not know what was going to happen and the work was continuing on. We were sort of sitting in limbo.

VM: Were you already associated with Calvin? Was he part of that work?

BT: No. I wasn't associated with Calvin, but my research professor, Gerald Eyde Kirkwood Branch — a very nice gentleman — was a close colleague of Melvin's and I actually used in my research Melvin's DU spectrophotometer. He had one of the very first of the instruments made by the Beckman Instrument Co. and so I knew not only Melvin but all of his graduate students and the rest of that group; we were in the Old Chemistry Building. I knew Calvin well and many of the other people that were in the Department at that time, in the period 1942-1945. What's not clear to me is exactly when, but I can remember that it was either late November or December that Melvin assembled together a group of four of us that had been working in the Radiation Laboratory and were sort of floating on loose at that time. They included

Pete Yankwich, Jim (*James C.*) Reid, myself and, I think, maybe one other, but he did not stay with the group. He (*Calvin*) told us that he had talked to E.O. Lawrence and that E.O. Lawrence had given him the authority to create a new organisation (*in the Radiation Laboratory*) which would develop the manufacture of carbon-14 and develop ways of using it, the synthesising of compounds and to apply it to various problems in the biological and chemical world. He offered us basically all jobs, moving out of our old uranium chemistry job into this. Of course, I loved Berkeley very much and I was very happy in the environment and we all sprung immediately to this job. That was really the beginning of the group. So that I was in the initial formation of the group.

Melvin had worked with E.O. Lawrence on this, and John Lawrence had built a laboratory, the Donner Laboratory, and he had quite a bit of extra space in that laboratory. Because he (*John Lawrence*) wanted this development, and it's really not clear whether this was only E.O. Lawrence's pushing or whether John Lawrence also was pushing E.O. Lawrence because both of them recognised the importance of carbon-14 and the importance of developing this technique. So he (*John Lawrence*) assigned us space in the Donner Laboratory. This space had been used previously for the uranium chemistry work. So it had been set up as a chemistry laboratory space and I had worked there. Actually, I hardly had to change my location. I just continued on the same location and we just discontinued the uranium chemistry work and started in on this.

VM: This was the third floor of Donner, was it?

BT: This was the third floor of Donner Laboratory.

VM: How much space did you have?

BT: We essentially three-quarters of the main floor of Donner Laboratory plus a couple of counting laboratories downstairs in the basement.

SM: May I ask which year this was, when the group got together?

BT: I am trying to remember whether it was in December of 1945 so that we actually got started on it in January of February of 1946 — is that the correct time?

VM: Yes; that sounds right.

BT: OK — so let's go back to it. Therefore, this group was set up. Then there were certain problems that were almost immediately assigned. Pete Yankwich was given the responsibility for developing the counting techniques, the analytical techniques...

VM: For C^{14} ?

BT: ...for C^{14} which was considered very difficult to assay because it had such a soft β -particle and most of the detectors which they had, which were aluminium-walled or things like this, would not pick them up, and you had to use a mica window one.

VM: Question: did you have lots of C^{14} at that time?

BT: We had only very tiny amounts of C^{14} at that time. I was assigned the responsibility of the synthetic part of the work. There was another person, I am trying to remember

his name, who was primarily assigned the responsibility for making the C^{14} . Essentially what we were looking for was some material that we could put into the nuclear reactors and irradiate and it had to have a very large percentage of nitrogen and it had to have no strong neutron-absorbers in it. Beryllium nitride turned out to be one of the compounds that fitted that material. And so beryllium nitride was actually made over in...Gilman Hall in one of the upper floors and was canned into aluminium cans and these aluminium cans were sent to Oak Ridge where they were irradiated in a nuclear reactor. That doesn't sound right because I don't think there were any nuclear reactors in Oak Ridge. They were sent somewhere and they were irradiated in as high a neutron flux as they could get for a finite length of time. And then they were sent back to us. In the very early days we had the responsibility of taking these aluminium cans, which were very highly radioactive, cutting them open, getting the beryllium nitride out under conditions in which you could catch the gases which were coming out, and then dissolve the beryllium nitride and collecting the CO_2 , precipitating it then as barium carbonate.

VM: Two questions. Did you remember who it was in Gilman Hall who made the beryllium nitride?

BT: I can remember it but I'll have to think about it. He became a professor over here in Kansas or Nebraska.

VM: Was he one of the Calvin group actually?

BT: He really was part of the group but not directly. He was much more directly attached to Connick and the people in Gilman. That was more of a co-operative effort. (*Editor: might this have been Denham Harman?*)

VM: The second thing. You said that you started out with most of the third floor of Donner. What about equipment? Did you have any, did you have to build it up, did you have to make it?

BT: All we had was the standard kind of equipment that was left over from the uranium assay work — that was beakers, pipettes and...

VM: You just inherited that?

BT: Yes, we inherited those. Fundamentally, we had to start figuring out how to make these. One of the reasons, I suppose, that I got into the synthesis work was that we were working with gaseous CO_2 . When I had done my (*PhD*) research I had constructed several vacuum lines. That was sort of required of all graduate students in chemistry at that time, to learn something about vacuum line techniques. It was relatively easy for me to start setting up apparatus, working with liquid nitrogen, traps, moving stuff around.

The first efforts were directed basically toward learning how to count carbon-14 or detect it, learning how to make it so that we had available materials, and then learning how to handle it and synthesise it and put it into forms that could be useful.

The photosynthetic work actually didn't start at the beginning of the group. These three groups started and the photosynthetic work didn't get seriously underway, as far as I remember, until Melvin brought in Andy Benson who had been excluded (*from the Radiation Laboratory*) because of his status during the work as a conscientious

objector. He joined the group and was assigned, I think almost from the beginning, a space over in the Old Radiation Laboratory.

VM: Now Andy had been associated with Ruben and Kamen before the war, or before things got tough in the war, and he had actually been part of the early photosynthesis work. You mentioned earlier in conversation that you also knew these people, Ruben and Kamen.

BT: Yes.

VM: What is your memory of them and anything of the photosynthesis that they did. Did you observe it? Were you part of it?

BT: No. I was not part of it. I knew Ruben because he was my PhD advisor when I was going through that work. I liked him very much; he was a really nice man and wonderful to work with. Unfortunately, he was poisoned by a phosgene experiment that he was doing and I remember very distinctly at the time because we were right there in the building.

VM: You were there when it happened?

BT: Yes. But I did not actually see it. I was in one of the rooms (*in Old Chemistry*) and was told almost immediately that this flask of phosgene had broken apart and, of course, the warm materials going into the liquid nitrogen caused the phosgene to be blown up into his (*Ruben's*) face, and he got a breath of it. Sam, knowing the toxicity of the materials, knew that he was in very serious trouble immediately. He walked outside, told somebody to get help and laid down on the grass to wait for help and they took him up to Cowell Hospital, which was only about half a block away and, unfortunately, it was a fatal dose. His lungs filled with water and he drowned, basically, as I remember it. They commented his lungs just filled with water, they couldn't do anything about it and so he was lost at that time. That was the end of that work.

This was relatively early on, by the way. This was while I was still a graduate student, in my early graduate student days, before I had started working on the other laboratory (*i.e. uranium problems*).

VM: So his death, while a personal shock, did not interfere very much with your subsequent thesis work?

BT: No, because I was working on absorption of triphenyl methane dyes under Branch, and using Calvin's equipment, and it had really no effect whatsoever, except that I lost a very fine and very nice mentor. I only knew indirectly of his work on the photosynthesis that was going on at that time which was talked about. Then, of course, it was continued to be talked about, because it was a very exciting discovery that he had made, and so then Melvin, recognising the importance (*of that work*), had planned a "do" on that (*i.e. planned to continue it*). He actually didn't discuss that in our initial conversations, that he wanted to go into the photosynthesis work. He merely said that we wanted to develop the use of carbon-14 as a tool in biological and chemical studies, and that we had to figure out how to make it, how to count it, how to synthesise it into compounds, and it wasn't perfectly clear that his long-term objective was to go into the photosynthesis work until he got hold of Andy Benson and brought Andy in and said "OK, let's establish the group to do the photosynthetic chemistry".

VM: He must have been thinking about it.

BT: I am sure he was thinking about it. But it did not come up in this initial conversation.

VM: So there were the four of you, then, the original founders of the group, working in Donner in these various areas. But you didn't stay just four people for very long, did you?

BT: Oh no., because there were plenty of funds available and so almost immediately, why we started to get assistants. I wish I had more early Quarterly Reports and that so I could go back and see exactly who were the people in the very, very early days. But, certainly Rosemie was brought in relatively early.

VM: That was Rosemarie Ostwald — or subsequently called Rosemarie Ostwald.

BT: That's right, and she was a refugee and Melvin brought her in. Melvin had a lot of very good contacts and he was quite perceptive on who was capable of doing interesting and good work in terms of the hiring. During this time I was gradually assuming a certain amount of administrative responsibility with respect to the group and, as you will notice, I was writing the Quarterly Reports in 1948, which is three years later — but I had been writing reports early on — and I guess that I assumed it because it was an activity that I enjoyed and carried on. And so as time went on, I became more and more involved in the administration of the group but, at the same time, I carried on a very active research programme and that sort of was a thing that I liked.

VM: Can we talk a little bit about these Quarterly Reports because, sitting on the table here, there must be two or three dozen of them from as far back...I think the first one says '47 doesn't it?; '47 to-November '49, and then they go up much later.

BT: Here's two more boxes over here!!

VM: You must have well over 50 of these things. How did they start and what was their purpose and who do you think read them, apart from you guys who wrote them?

BT: Well, that was quite clear. They were required by the AEC (Atomic Energy Commission) office in Washington, DC which was supplying the money to us. They wanted these Quarterly Reports. I think in addition to that, E.O. Lawrence himself also wanted the Quarterly Reports so that he would know what was going on. The circulation, as far as I know, was to the administration of the (*Radiation*) Laboratory and to the Atomic Energy Commission in Washington, DC, to the Division of Biology and Medicine.

VM: And you think people read them there?

BT: Oh yes, I'm sure that they read them.

VM: Did they ever comment back to you about anything that you had done, or had written?

BT: Not in specific (*terms*) but they did visit the laboratories on a regular basis.

VM: Were these classified at the time?

- BT: No, they were not classified and our work was not classified. Calvin was rather insistent from the very beginning that our work be not classified. It is true that all of us had security clearances and the new people that we were hiring at that time had to have security clearances. But the work was not classified, the laboratory was open, we could have foreigners, as you know, working in the laboratory...
- VM: Although even there, there was some sort of scrutiny undertaken and they were given special permission...
- BT: Perhaps.
- VM: Yes, there was something of that sort.
- BT: OK, so you remember that. I remember that the security clearances was something that we had to always abide by. In fact, there is almost a funny story on that. We were getting along fine; I had one assistant that was working in the biological chemical area, and, finally, the security people came to me and they said "we don't really like this lady working in your group; we would like you to let her go". And I said "but I don't want to, she's doing OK and what's the trouble?" They hesitated to tell me, but finally they did. It came out that she had a lesbian relationship with another lady and they felt like this was a security risk. But I think I was rather adamant and wouldn't fire her. So finally we reached an agreement in which she would have no access to any kind of classified material and we would avoid anything that contacted it and she was retained. That was, for instance, one of the minor incidents that arose.
- VM: Don't tell us any secrets, but was there any classified material actually involved in the group?
- BT: Yes. I had a number of documents relating to the uranium chemistry (*project*) and I had available to me a certain amount of information relating to the uranium chemistry. Then, on various kinds of trips that I went on, to see other national laboratories and coordinated with them. I visited Oak Ridge, Los Alamos and all of these place, even up in Washington to the Richmond laboratories. In all these cases, why I heard and got more information about that. So I had documents but not everybody did. Most of the group had no security documents. (*Editor: Note that Calvin himself also had a considerable quantity of classified documents relating to his own war work in the Manhattan District and the plutonium extraction process.*)
- VM: That was the only classified material that you came across. It wasn't directly related to the work you were doing (*on the carbon-14*).
- BT: None of the work that we were doing was considered as classified.
- VM: If I look back at the earliest report that you have (1947), I think you mentioned (before we started taping) that this one dated, well '47-November '49 — but anyhow, this particular one is September/October/November '49, and you think this might have been the first or...?
- BT: I think that there's an earlier one and I would like to find it. I'm almost certain that there is. I'll have to go back through some of my boxes and see if I can find another one of these black binders.
- VM: You think that these reports started fairly early on in the life of the group?

BT: The reports started very early on.

VM: They were, of course, hand typed and corrected. I notice they are on the old erasable paper which is no longer used.

BT: It was a terrible job for Norma (*Werderlin*) and Marilyn (*Taylor*) to type these things up. They had basically mechanical typewriters and they had to hit them as hard as they could to try to get, I think, the 7 or 8 copies that we could make and if they mistruck one, they had to erase down through the 8 of them, using protectors. It is incredible the difference that photocopying and now the computer has made in our ability to transmit information. I think I kept them (*these reports*) if nothing else because they are really such rare copies. There were 8 copies of these things made and this is one of the 8; where the other 7 copies are, I don't know.

VM: I think it would be very nice if these were properly archived eventually so that they were not lost to history.

BT: One of the things that is really relatively interesting is that these documents here, and then a relatively simple document prepared once a year for the Atomic Energy Commission outlining the various groups and what their goals were and so forth, was all that we needed in order to get the money that was being used by the group, which was a substantial sum at that time. The lack of paper work associated with the funds in this work was an incredible pleasure after having lived through a later professional career where you had all kinds of grant applications and an enormous amount of paper work to put out.

VM: It tells a lot about the relationship that must have existed at that time between the Washington people and the Radiation Lab. in Berkeley, and, I guess, Lawrence in particular and the hierarchy down from him.

BT: One of the things, though, that was very interesting, was that the people in Washington, DC were sophisticated scientists at that time. Many of them were not the classical bureaucrat. They were good scientists. They made sure that they got people who were competent and many of them — I even spent a year there, you know one time, working in the Biology and Medicine Division reviewing grants and contributing to that. They tried to get outside people in so that they wouldn't keep a bureaucratic perspective. They were very nice and they knew what was going on and they appreciated it. And they had the money. Congress had given the money and very generous with it.

VM: So, you had easy personal contact with these people, did you? You knew them yourself? If you needed something, you could call them and discuss it.

BT: Yes, that's right.

VM: The names on this very first sheet, as it were page 1, Act I, Scene I, Calvin and you — those were the first two. Some of these other people; I didn't know them, of course — Pat Adams?

BT: Pat Adams was a lady who got her degree in Berkeley, was a very charming lady and did synthetic work. I can even tell you a couple of stories about that which might be funny. Because we were working with radioactivity and all that, we always wore white lab. coats, we were required to wear white lab. coats. Quite often I would be down in the laboratory and would be talking about some given problem and we would

want to write out some formulas or something, and I might not have a pencil, and they might have a pencil and I guess at least once, if perhaps not more times, why I'd grabbed a pen out of her pocket and helped her write down that. Well, I found out sometime about a year or two later that Pat had made the comment "just as long as he only grabs the pencil!" She was a very charming lady and I liked her. The other thing that was sort of interesting and almost sad about Pat but really, in the long term, it had no (*effect*)... One of the people there is Art Fry; and Art Fry after he finished is a graduate student and was working on the decomposition of acetyl peroxide which is a very unstable compound and can explode violently in a detonation type of explosion. He is now, by the way a retired professor of chemistry at the University of Arkansas in Fayetteville. He had used one of the vacuum lines that we had used and had distilled accidentally over, maybe he knew but he didn't realise the consequences, a small amount, probably like a half a gram, of the acetyl peroxide into a trap and then had taken off the liquid nitrogen trap and that was finished for the day. And so the next day, why Pat Adams came in and was going to use the laboratory for something else and she reached up and grabbed the trap to take it off to clean it and the acetyl peroxide detonated. Now, when it detonates there is a shock wave that goes out at greater than the velocity of sound and the glass just shatters into a million pieces. She got a few small cuts here and was not seriously injured but it was really a frightening experience for us. I remember that John Weaver, who was an MD, quickly came up from one of the lower floors (*of Donner*) and administered whatever (*was necessary*). I don't think that the cuts were so bad that they caused any disfiguration. And, thank goodness, none of them hit her eyes; I suspect that she had glasses on but she did have some cut marks on her face.

VM: Did people wear safety glasses in those days?

BT: They weren't required to. That's why I hesitated whether she had safety glasses on. It was considered good as a chemist to be nearsighted in those days, so that you had to wear glasses all the time, because it was safer.

VM: People, of course, in those days were much less safety conscious altogether. They used glass pipettes (mouth pipettes) and nobody seemed to worry too much.

BT: Yes and no. In our laboratory whenever we were pipetting with radioactivity, I introduced very early the concept of using a vacuum suction tube or a suction bulb to pipette that. Because when we were working with uranium chemistry earlier, why we never used mouth suction to pipette the radioactive materials. So even at that time we did not use mouth...I mean, we used pipettes but we wither used a suction bulb or we used a vacuum line, closing it off by the amount of air that whistled by.

VM: Did you have protective shields when you worked with radioactivity, did you work behind, stick your arms around shields?

BT: No, we did not. Remember that carbon-14 is a very soft radiation and there is essentially no radioactivity which goes outside of the flask and only if you have a large, large amount of radioactivity do you get something called "Bremstrahlen" which are soft x-rays which will go outside of the flask. The only hazard, basically, in working with carbon-14 was the possibility of ingesting it or breathing it, so we used gloves to a limited extent but not completely. No. we did not work behind shields; we did work in hoods.

VM: What about safety measures? There must have been...No, perhaps I should ask and not just say "there must". Was there a safety organisation associated with the work?

BT: Yes. There was a safety organisation and, after our initial efforts, which were relatively simple and which we had created over the first three to five years (a very extensive network of vacuum lines, and so forth), the safety engineer — I am trying to remember his name and maybe I can't (*Editor: it was Nelson Garden*) was looking at it and he said "I don't like at all, all of these vacuum lines working with radioactivity in relatively large quantities". He proceeded to see what we were doing and he dreamt up a system in which we created boxes, basically on wheels, in which the vacuum line was held into it, it had sliding doors in the front and it had an exhaust fan to exhaust the material out from it. And so then, after about the first four or five years, why we started to work with these vacuum lines in boxes, which was a very good technique. But in the early days we worked with it right out in the air.

VM: Were there any serious accidents?

BT: No. There were no serious accidents. We never had any evidence of anyone getting any contamination of radioactivity. We tested to a limited extent the urine and the breath of the individuals.

VM: Did people wear film badges?

BT: Yes, interestingly enough. We wore film badges and they were all negative always because none of the radiation of carbon-14 would go through that. That was considered as a formal precaution against law suits more than anything in terms of a real health safety measure.

VM: OK, to come back to this list of people: What happened to Pat Adams, do you know?

BT: Pat Adams eventually married somebody, a nice person, raised a family, and then she went back into some kind of chemical work in the area. (*Editorial note about Pat Adams: she was married when she came to the group.*) Winifred Tarpey, who...

VM: She's not on that one but she's on some of the others.

BT: ...she's on the next one, who was hired at almost the same time, she might have been in the other part of the group here, let me see...do we have Winifred Tarpey?

VM: She's in one or other of those sections.

BT: Yes. Actually, Melvin hired most of these people (it wasn't until later that I began to interview them) as recent graduates from Berkeley in Chemistry who were looking for jobs.

VM: Did he discuss with you, and with the other three of you who started the group up, how it was going to expand or did he just walk in one day and say "this is so and so who is joining the group"? How did this happen?

BT: There was discussions about what our needs were, whether we needed more people. It was recognised, for instance, in this period of time, that we needed more people to do synthetic work. We have various things that we were trying to make in good quantities and good yields. At that time, the carbon-14 was considered very expensive, and it was; I think we were paying \$32/millicurie when Oak Ridge started to make it. And although \$32 seems like little money now, that was in the days when you could buy a new car for \$600.

VM: So, you actually had to pay for this stuff even though it was made internally in some way?

BT: Yes, out of our budget. After they started making it in Oak Ridge by radiation, either of ammonium salts or the beryllium nitride irradiation, why then we gave up the effort very quickly and happily. Because we didn't like working with those hot aluminium cans that had been in the reactor.

VM: And the price fell a lot, I suppose.

BT: It continued at that price for a long, long time. It was a significant item in our budget. In fact, we had to transfer funds to them.

VM: Initially, I suppose, all the hot carbon came to you as barium carbonate.

BT: Yes, it all came as barium carbonate.

VM: You were among the pioneers of converting this stuff to anything else.

BT: That's right.

VM: And indeed collectively — I don't remember who the authors were — there was a book on isotopic carbon which included a lot of this information.

BT: That's right. And I have a copy of the book downstairs if you want to look at it.

VM: You were one of the authors?

BT: Yes, I was one of the authors.

Pause to fetch the book

VM: We now have the book ("*Isotopic Carbon*"), and, as you pointed out, \$5.50 was the original price, published in '49! It must have been written...if it was published in '49, it must have been written in the period '47-'48, I suppose.

BT: Yes, it was written basically in '48.

VM: All of this material, all this experience had already been accumulated pretty quickly.

BT: That's right. We moved forward very, very rapidly on all of these phases, as rapidly as we could. We had excellent resources in terms of money and people. As you will notice, the authors on it are the three original group members — namely myself, Pete Yankwich and Jim Reid and then Melvin — and Charles Heidelberger had joined the group, after it had gotten started, on the biological end and he contributed enough to it and wanted to be part of the book. So he joined the book group's authors.

VM: This book — of course it's in the record, but just to get some information here — this book has a lot of information on technology — how you do things — with respect to carbon-14...

BT: Yes, that's what it was.

- VM: ...and then it also has things on synthesis and degradation methods actually of chemicals, doesn't it? It was hundreds of pages...a major effort and I think it has actually become *the* classic in the field.
- BT: Certainly for something like ten years after that, this was the standard reference work to it. Within ten years after that there were major new documents produced.
- VM: Can I ask you something? When it came, a little later on, to measuring the radioactivity on chromatograms, were those pieces of equipment things that you did as well, those Geiger counters and those flow...those Scott tubes (do you remember those?) with mylar, gold-sputtered mylar windows?
- BT: The gold-spotted mylar windows were commercial developments. The initial developments that we did in the group: Pete Yankwich made them (*the windows*) by splitting mica until it became very thin and evacuating them and filling them with the counting gas.
- VM: Did he glue the mica onto the..? How did he attach...?
- BT: He did it with hot Apiezon wax.
- VM: So he could get it off again if the windows got broken?
- BT: Well, the windows got broken. And, yes, they could be replaced and were replaced.
- VM: At some stage you moved then to the mylar windows which were just held on with O-rings or rubber bands.
- BT: They could be, or they could be also waxed on.
- VM: When did that happen, do you know?
- BT: That was later. By the end of the forties, you see, an enormous amount of this technique had been developed and formalised and incorporated into the industrial world. All that mylar stuff was commercial tubes.
- VM: But the design of the Scott tubes? I don't know who Scott was — but do you remember those with gas flow, hooked up to the helium cylinder...?
- BT: That was a development, I think, that New England Nuclear, no — I am sorry, that's not right. That was a development that was made by some people at either the Argonne National Laboratories or at Brookhaven, or a combination of those.
- VM: Because the counting chambers themselves were clearly not commercial. They were home-made in some sense, whether in Berkeley or some place.
- BT: We were still making them. I take it that you used some of those.
- VM: Oh, indeed I did, yes, together with all the other people.
- BT: This ("*Isotopic Carbon*") also contained determination of counter efficiency. Pete Yankwich would have written that one, statistical treatment of counting data, Pete Yankwich would have written that. The work on flow and vacuum systems, manometers and gauges, that I did. Stirrers for vacuum systems, which we didn't

know how to do, that was my developments there. How we went about doing these various things: I have also some much later publications, big, big books of all the synthetic methods which were accumulated. Those actually came out of the Los Alamos National Laboratories where they accumulated (*these techniques*). Remember, that after we got started on them, it was probably less than a year before groups picked this up at Oak Ridge, at Argonne National Laboratories, at Brookhaven National Laboratories, and somewhat later, but still in the same time frame, it was picked up by work at Richmond (up in Washington) Laboratories — Richland, I'm sorry — and down at Los Alamos.

VM: So initially, all the C¹⁴ was really confined to government labs., was it? It was only later did it percolate to others?

BT: Well, we would synthesise materials and give it to other people...

VM: ...in the university structure?

BT: ...in the university structure. A lot of the stuff that we made went out to them for their uses.

VM: Incidentally, before we finish with the "*Isotopic Carbon*" book, what did Melvin do for the book? He contributed a great deal to it. He wrote parts of it and he was the one who brought the idea to us and said "look, we've got to get a book out covering these various parts". I can't tell you offhand just exactly who wrote all parts of this material.

VM: But he wrote some of it as well?

BT: Yes.

VM: So back to these names, then; we've got a lot of them. Bartsch: who was Bartsch?

BT: (Bob) Bartsch was a graduate student and I don't know where he is now.

VM: Art Fry?

BT: Art Fry I just described.

VM: H. Gray? Well, if you don't remember, you don't. The next one is a D. Hauptmann?

BT: As I remember, Hauptmann was an older man who was visiting the laboratory on sort of a special postdoctoral type of status or a sabbatical status. I don't remember more about Hauptmann. (*Editor: He was from São Paulo, Brazil*).

VM: That brings me to another question. This group started with four of you and then it grew to start with by Melvin hiring people. When did the graduate students and the postdocs. begin to be part of it?

BT: They were all Melvin's graduate students because none of us had academic status and so all the people who came in as graduate students were associated with Melvin. Melvin outlined the projects for them. They, in general, did not do the synthetic work which was considered not worthy of a thesis necessarily. It was necessary work that had to be done. And Bob Bartsch and Art Fry and the rest of the people were Melvin's graduate students.

VM: But working in Donner with you?

BT: Yes, working in Donner with us. I think that my name is even on one of Art Fry's papers or something else like that because I worked with him rather closely.

VM: Since you were there all day every day, as were the graduate students, you in fact must have been working with them.

BT: In a way, you know, we were like super postdoctoral assistants working with the graduate students.

VM: Incidentally, one of the things was, when you were hired by, I guess, the Radiation Lab. was your formal employer,, you were hired simply as a permanent employees without time-limited things, as far as you know?

BT: I was hired by the head of the Chemistry Division, as I say, and I think it was in January of — let's see, I started my graduate work the fall of '42...in the fall of '43...in January of '44 I was hired by a guy by the name of Prescott into the Chemistry group at the Radiation Laboratory. That was an entirely different group, that was a group that was working on the uranium chemistry.

VM: This was just an open-ended employment.

BT: It was an open-ended employment. We actually didn't ask whether it was permanent or anything else. It was a wartime job. You see what really happened was that we were graduate students and we were exempt from being drafted at that time. As the war efforts intensified, and as the need for chemists in the Manhattan District increased, why, then there came a time when they said "OK, you can't be a graduate student any longer; you've got to go to work full-time" for the Manhattan District...

VM: ...even though you hadn't completed your PhDs?

BT: ...even though I hadn't completed a PhD. Actually, it was interesting that from that time I worked two full-time jobs. I worked a full 55-hour job in the Radiation Laboratory on the uranium chemistry and in the evenings and on weekends I would work on my PhD thesis research down in the Chemistry Building. That was really no problem because there was nothing else to do. There was a war on, there was a limited amount...you couldn't travel, you couldn't go anywhere and it was exciting.

VM: Other people were presumably, were in a similar position, so there was a group of you.

BT: That's right.

VM: OK; so that's as far as Hauptmann. Do you remember any more of these? This one: S. Hughes? (*Editor: Probably Dorothy (Hughes) Johnson, BS chemist in the Donner Lab. who did synthetic work prior to Pat Adams.*) If you don't, let's not waste time.

BT: Saburo Ikeda. I just remember the name. Jorgensen (*Editor: Eugene Jorgensen had the distinction of being the only member of the group who was ever murdered!*), Bernie Neivelt was a postdoc.-type of gentleman from Switzerland and I'm sure that you could run him down. Rosemie (*Rosemarie*) Ostwald was a very nice lady who had taken a PhD in organic chemistry from Karrer in Zürich and was a refugee from

Vienna where her family had been. Melvin had met her somewhere or other and she joined the laboratory and she was a great contribution. Bob Selff, I think was another graduate student, but I would have to look. (*Editor: Bob Selff was a BS chemist like Pat Adams.*) Yvonne Stone, I can't remember her. (*Editor: She was a BS chemist who worked with Pete Yankwich in the counting room and on counting procedures/developments.*)

VM: The next thing that occurred to me that I would like to ask you is the communication inside the group. By the time I got there, eight years later, the Friday morning seminars were well developed.

BT: They were developed then; they were going.

VM: From the beginning?

BT: From the beginning. From the beginning we had weekly seminars and everybody met and Calvin would ask various people to talk about what they were doing and give reports. You didn't prepare for them at that time; why, he just went wherever he wanted to. That kept the group together. That meant that everybody knew what was going on and heard what was going on.

VM: By that time, by the end of a few months at any rate, you had, what?...a dozen people, or some such number as that? It had grown from your original four.

BT: I said that we had this organisational meeting in late November or December (*of 1945*) and we got started on this thing here at...I would say that, yes, we probably had...maybe we had a dozen people. After all, we had Calvin's graduate student group...

VM: ...and there were several of those?

BT: Yeah, there were several of those. And as the new graduate students...see, at that time, there were a lot of GIs coming back coming back from the war, and they were very good, and very hard-working and older people. So Calvin was adding new graduate students to his group at a pretty good rate.

VM: If you try to remember what one of the early seminars looked like, did you have a room full of people or were there just a few people sitting around the table?

BT: OK, well that's a way of finding out how many people...my memory of it is that there would be the equivalent about two or three library tables and they would be pretty much surrounded. I would say that there must have been by the end of the half-year at least a dozen or more people.

(Tape turned over)

VM: We talked about the period which must have been the beginning of 1946ish, after the group started and there these dozen-ish people. And you were all in Donner. Eventually there was a second leg to this group developed in ORL.

BT: That's right.

VM: How did that happen?

- BT: Basically, as I remember, Melvin hired Andy and they had to find some additional space for it (*i.e. for the photosynthesis work*) and the cyclotron group had been contracting over there and there was space in ORL. And so E.O. Lawrence gave that space to Melvin, and Melvin and Andy then started to rebuild it immediately and equip the laboratory and get started on the photosynthesis work. Andy spearheaded and drove that effort.
- VM: So Melvin had the resources, presumably, at that stage, to equip another building.
- BT: Yes, no problem.
- VM: And put people in it as well?
- BT: And put people in it: yes.
- VM: Now, once that began to happen, what happened to the relationship between you and the others in Donner and this other group in ORL?
- BT: In the first place, the relationships were very cordial. We were all part of the same group. Marilyn and Norma and me in the office and Melvin integrated the entire group together. We met weekly together for the research conferences so they knew just as much what was going on and listened to the conversations as that. The only thing was since we were not specifically excited by that work, because we (*in Donner*) were trying to solve our own problem, we listened but didn't absorb it on the level that we would (*have done had it been our research problem as well*).
- VM: The group remained, you think, one group, administratively and financially?
- BT: Yes, not only did it remain one group administratively, socially, and remember that that group (*in ORL*) was calling on us for counting techniques and for the barium carbonate or the CO₂ that would be prepared in our group (*in Donner*) and all the rest of the activities like that.
- VM: There was a period, which must have lasted for some 16 years or so, from the beginning of ORL, which I guess was '47-ish, or something like that, until they all moved into the round building together in '63, when there were actually two groups, or, at least, two locations. By '56, when I got there, there was some feeling of difference between the two groups. The Donner people...you didn't see them so often, if you were in ORL, you knew them, but you didn't know them as well.
- BT: That's right.
- VM: Did this feeling start earlier? Did you feel the same thing from Donner?
- BT: Well, we never felt like they were a separate group; we merely felt like they were a separate phase of the research. After all, by that time, Ed Bennett was going his way relatively strongly and other kinds of work was going on (*in Donner*). I was working extensively with the MDs in Donner, trying to do respiration measurements of CO₂ with a guy by the name of Nathan, N.I. Berlin. We were using labelled glycine to determine the turnover time of the red blood cells in the body. We were just different types of approaches. But Melvin, by holding the research conferences and everybody going every week, made sure that all parts of the group remained adherent. I felt even at the time that I left, which was in 1957, that although there was distinct groups, that socially they were highly integrated.

- VM: From your position in Donner, and conscious of the fact that you were one of the founders and therefore you were more likely to have known everybody than the later joiners...
- BT: That's true, that's true.
- VM: So, the young people who joined (*Donner*) in the fifties, say, would not have known the ORL people as well.
- BT: That's right. They would have gone over there and they would have had relatively little contact with them. My wife, Anne, was there (*in ORL*) as a graduate student and I think that she hardly knew the people over in Donner because she was a beginning graduate student and didn't go over to where we were. She was having strange enough surroundings to be in where she was without coming over to this other building full of MDs.
- VM: As you say, socially they were very well integrated.
- BT: Yes. A lot of that was due to the efforts of Gen Calvin and Marilyn who was a very strong force in keeping them integrated, and Rosemie (*Ostwald*). There were organised skiing parties and other kinds of parties and Christmas parties and everybody got together for these social interactions which was very important in creating a continuing bond between the various segments of the laboratory. I have lots of pictures of some of these old parties and they are a pleasure to behold.
- VM: Calvin was clearly the scientific originator and leader of the thing, but he was not really a manager, was he?
- BT: First, I think he really didn't want to waste his time doing the managing. I think that he could certainly have done it. I had an aptitude for handling the management affairs without getting into too much trouble and so I gradually assumed more and more of this kind of responsibility. It was relatively early, almost from the beginning, that I started to write the quarterly reports...or not write the quarterly reports — I got pieces of paper from each one of the various segments of it (*the group*) and then tied them together, put introductory notes on them. You notice I say here (*on the quarterly reports*) "edited by me (*B. M. Tolbert*)". In other words, I'm not claiming that I wrote this thing here but I merely edited it and the source of the various pieces of information came from the individuals.
- VM: What about things like the budget. Did you have to...you said that there was a relatively simple way of getting money from the AEC, but nevertheless it had to be done; was that your responsibility?
- BT: Yes, that was my responsibility. I handled the budget and basically, between Calvin and I, why we handled the salary increases and adjustments to the salary and all the paper work that went on the administrative level basically went through my office. Melvin was smart enough to know that he wanted to concentrate on the science and I was willing and able to handle this other kind of work without getting into too much trouble.
- VM: He signed things that you put in front of him, presumably, which were relevant?
- BT: Yes, that's right.

VM: What about equipment and supplies? How were they purchased; who was responsible?

BT: They were purchased by purchase authorisations which went through our office and which I signed.

VM: It was a relatively simple matter, wasn't it, for small amounts of materials.

BT: Oh, yes.

VM: On the nod, more or less. What did you do about bigger pieces of equipment?

BT: I don't remember where it was that we couldn't (*sign things ourselves*), but I don't remember having any problems at all with buying almost any kind of a piece of equipment. Geiger counters and things like that were some of the major pieces of equipment; vacuum glassware, there was more than adequate funds to take care of everything. I think if we had gotten into trouble and needed more money that E.O. Lawrence would have footed the bill and said nothing about it. So at no time do I feel that we were really constrained on the basis of budget.

VM: It was a most favourable situation which hasn't lasted, unfortunately.

BT: (*Laughter*) It was a wonderful situation in that respect to have interesting scientific problems to be tackling and sufficient resources to develop them at the maximum rate.

VM: Do you think that sort of group — that group — could have developed if the money not flowed as easily as it did?

BT: I think so. I think it would have been somewhat different and it might have been distinctly slower and things like that. But I think that the breadth of interest that Melvin brought to it...remember it took a wide breadth of interest for Melvin to be interested in everything all the way from the biological type of work that Heidelberger and Ed Bennett, and really interested in what they were doing, and then the synthetic work which we were doing and then the photosynthetic work on Andy's part. At that same time, may I remind you, he was also deeply involved yet with some aspects of chelation chemistry and had written some books on it and was continuing to be active in that. That was going on not through the group but all but through his chemistry (*department activity*).

VM: One of the points in relation to this — we were talking a few days ago to Sam Aronoff who said that he came back on visits to the group fairly late on in his period, that is to say, I suppose, late forties and maybe even early fifties — (*and Calvin was*) still working, I think he said, on cobalt oxygen chelates.

BT: Yes.

VM: Was that going on that late? That was, I think he said, for possible submarine use to supply oxygen?

BT: Yes, that was probably going on. But this part of Melvin's (*work*) had no activity in that (*i.e. no connection with the Bio-Organic Group*). That was strictly over in the

basement of the Old Chemistry Building. And I didn't think that very much of that was going on, any more at that time. It was mostly paper work.

VM: So Calvin had activity in the (*Old*) Chemistry Building which was separate from ORL and Donner?

BT: I don't know the details of it but I suspect there was some. A graduate student, a few graduate students over there. After all, during the war effort he had a relatively large ONR contract on this work and had quite a large group of people that were engaged in synthesising all different versions of these (*chelate*) compounds and looking at their oxygen absorption properties.

VM: That was in the Chemistry Department in Berkeley?

BT: That was in the Chemistry Department in Berkeley. In fact, down in the basement of the Old Chemistry Building, because that was where Gerald Branch's laboratory that I was occupying, was down there too.,

VM: Did you see a lot of Calvin? Did Calvin come into Donner very often?

BT: Yes, he came into Donner quite often and then I would go to his office.

VM: Did he come into Donner as a breeze in and breeze out, or did he spend a lot of time, did he go through people's individual results?

BT: No, he didn't. That basically was done at the weekly research conferences when those things were covered. In terms of our work, no. He might come through occasionally to see what we were doing and keeping track and give whatever suggestions that he could, but basically we were carrying that work on ourselves.

VM: When it came to questions of publication in the open literature, you made decisions yourself, did you, about publishing, that this was the time to write something up?

BT: Yes. And if we wanted consultation and advice, why we'll talk to Melvin about it, too.

VM: Did he insist that his name went on everything as a matter of course?

BT: No, not at all. If you will look over here, for instance, in this volume I have "Synthesis of Labeled Compounds 1947-1950", here are a great many of them. You will find that although here is one that Melvin Calvin's name is on, you will find that Melvin is not on most of those names (*i.e. publications*).

VM: These were things that he didn't therefore insist on getting himself involved with things that he actually hadn't contributed to.

BT: That's right.

VM: You were very involved: there must be hundreds of things, I guess, in these things (*i.e. the Quarterly Reports*) that you were involved with here (*in this volume*) and there are lots and lots of people involved. I can see that.

BT: He was a very good director in that respect. I mean, when it was his graduate student that was doing (*the work*) like, for example, Art Fry, why then Calvin's name would

go on it because that was a different situation. But when it was Charlie Heidelberger's work or when it was Jim Reid's work or my work, and if we were just carrying it on and doing it, why we were treated as mature scientists in our own right.

VM: The graduate students probably welcomed having his name on their paper because it probably helped their careers.

BT: Yes.

VM: In those days, at the end of the forties and beginning of the fifties, there were the more senior people who were, I guess by then, you and Dick Lemmon and Andy, Al and Ed. That was the group of senior people who essentially worked there permanently.

BT: Yes, that's right, as long as they wanted to.

VM: There were clearly some support people: there were secretaries and so on who had the usual type of secretarial employment, and then you had technical assistants. They were also on permanent employment (*status*)?

BT: Yes, really, very much so.

VM: As well as support staff — dishwashers, store keepers and...

BT: Typical of that were, for instance, Martha Kirk and Ann Hughes. Martha Kirk was hired... Now, you see, the support people I might have helped hire or I might have hired myself, but even if I did decide to hire them, before I would formalise it I would take them over and Calvin would meet and talk to them and then give his opinion on where it was. For instance, Martha Kirk was hired and she worked for several different groups. For a long time she worked with me doing animal work of various kinds. After I left the group, then she went over and joined Al Bassham. There are a lot of papers of Bassham and Kirk in the later stages of the photosynthesis work.

VM: The group was always a group of the Radiation Lab., wasn't it?

BT: That's right.

VM: And so you were answerable to the Radiation Lab. administration in whatever form.

BT: That's right.

VM: So that means if you and Melvin between you agreed to hire someone, you then had to hand it over to the Rad. Lab. people who did the paper work on that.

BT: That's right.

VM: That went for all of the things, although you were always based on campus, it was always a Radiation Lab. group.

BT: It was always a Radiation Lab. group. and not part of the Chemistry Department, which is interesting.

VM: What do you recall as being the relationship between this activity and the Chemistry Department? What did the Chemistry Department think of it, as far as you could tell?

BT: That's a very interesting question. It was obvious that they approved of it and had it but it's not clear that Melvin had as much support in the Chemistry Department as he necessarily wanted. I always felt that one of the reasons that he supported Rapoport's work all through these years, providing him with at least a postdoc. and an assistant or two and a certain amount of money, was that Rapoport as a colleague gave him support within the Chemistry (*Department*) on the professorial level. Melvin actually wanted faculty members involved but there was not a lot of them that got involved with his group. It wasn't until later periods when you got...

VM: Tinoco and Hearst.

BT: Yes, and these people involved...that there became more faculty involvement. In a way, you can say that these people got involved because here was a source of funds and this was important for the continuation of their research because as funding got tighter...In the early days, everybody in the Chemistry Department really could get all the funds that they wanted to carry on their research. You might call those "golden years" from about 1945 up until about 1965.

VM: If they participated with Calvin, it was because there was an academic benefit to both parties. What about people from other departments? Clearly Calvin as a member of the Chemistry Department would have his closest links with them. Was there any...and I don't recall any of this but you would know. Was there any attempt to involve the Biochemistry Department or the Botany Department or any other academic groups on Campus?

BT: Yes, there was, and that was engendered basically through the individual people. Ed Bennett set up an extensive collaboration effort (*with the Psychology Department*). Many of the people didn't know enough of those people. On the other hand, we did contribute to their research by supplying labelled compounds and techniques and information, but none of us set up close relationships with these people. I knew and interacted with a number of people down in LSB. See, at that time, Stanley's biochemistry building hadn't been built yet, although it did get built before I left...

VM: ...the one that's called the Virus Lab?

BT: Yeah, the Virus Laboratory. That was the first biochemistry building that was built on the campus.

VM: One of the things that I have always found a bit puzzling, and maybe you can help us understand a bit, is the relation between Calvin and Arnon. (*Laughter*) By the time I joined, we knew that there was a stand-off relationship but I don't actually know why. Do you?

BT: I actually do not know why. I cannot answer that question adequately. They were doing research in the same areas and they were competing. Arnon was a good researcher and he could produce some very interesting results, and he did. But Melvin obviously had more people and more group and more money at his command. I can remember that Al Bassham, who was very good at singing songs — an excellent voice — had written a number of long ditties about Dan Arnon and the photosynthesis story; perhaps you have heard some of those.

VM: Well, if so, it's a long time ago and I don't remember.

- BT: I don't know the origin of that, except it was just they were both working in the same field and competing and each one felt like they had special things.
- VM: You were aware of this antipathy or whatever it was?
- BT: Yes, that's right.
- VM: I don't know whether they actually disliked one another or they were simply scientific competitors.
- BT: What they might have done was, instead of remaining sort of secretive with respect to each other, they might have collaborated more closely. But I don't think that they wanted to collaborate more closely. I think that each one wanted all the glory and didn't want to have any ideas that might be inferred came from the other person.
- VM: They were a bit touchy about one another's possible encroachment.
- BT: Oh yes, they were quite touchy about that. As far as I know there was relatively little communication. I don't even remember, for instance (which I would have), that Al Bassham would have gone down and sat in on Dan Arnon's seminars and things like that which he probably should have.
- VM: Did any of Arnon's people attend Calvin's seminars?
- BT: No.
- VM: So, there wasn't that much collaboration.
- BT: There was no collaboration; and it might have been that just from the fact that there was no collaboration resulted in this feeling of competition.
- VM: As the thing (*i.e. the Bio-Organic Chemistry Group*) went on through the fifties, it got considerably bigger? I don't have a chart showing just what the rate of expansion was, but it was, I guess, by the mid-fifties, it must have been 50 or 60 people strong already.
- BT: Yes, that's exactly right.
- VM: Filling all the available space, presumably, that was available; it was fairly tight.
- BT: Yes; in fact, squeezing very much over in ORL where we didn't have as much space. And even in our area, why we were relatively tightly squeezed because John Lawrence's group by that time had gotten large enough that he didn't want to give up any additional space.
- VM: Bearing in mind that you left in '57, was there any consideration before you left about getting other accommodation?
- BT: There was no formal activity at that time. Nothing had been discussed on a formal level that I know of about building another building...
- VM: Or even moving into another building?
- BT: Or even moving. It was certainly recognised that there was not enough space there.

- VM: Calvin's group continued to burgeon all along through that period. People were pushed in. In fact I seem to remember that somebody or other said about the mid- to late-fifties he was beginning to have to curtail the number of postdocs. who came because there simply wasn't enough space for them.
- BT: I am sure that is correct. He had created a sufficiently great name and there were that many people (*who wanted to come*). One of the things that was always very interesting I remember during this period was the relationship to foreign postdocs. He had a large number of applications from people all over the world. He said "I have one criteria that I can select these people or choose them. I won't take anybody who doesn't get money from his own country, or his own government or from his own area". It wasn't that the money was what was important but he said "I want the decision that they think this person is important enough and good enough to come over here and the only indication that I can really get on it is the granting of partial support".
- VM: It's not a bad idea, when you have lots of people that you don't know, coming from places, from labs. that you don't know either. You have to have some way of sorting them out.
- BT: That was the criteria that he used.
- VM: I have to say, I think that worked rather successfully. People who came there were good quality people.
- BT: They were excellent.
- VM: There were very few who were not, if any. I don't remember anybody who wasn't. Did you feel that the postdocs...Let me ask a preliminary question. Postdocs. came to Donner as well as to ORL, did they, from all over, the international crowd? Did you feel that they contributed interestingly to the group, just apart from their scientific work, but the fact they came from foreign countries and different cultures.
- BT: Incredible contributions. The conglomerate of the various people and their viewpoints and everything made the group exciting all the way through. They were welcomed and there was no discrimination against them and they became parts of the group. It was absolutely marvellous. They were very important.
- VM: I suppose one of the things it has done is to produce an international network of these people now, at least through Western Europe, partly into Eastern Europe and in Japan where there have been X-members of the Calvin group, as well as, of course, all over the United States.
- BT: Remember at this time the world was shrinking because of the airplane had made travel relatively easy and fast and it was becoming rapidly less expensive. I remember that in 1952 I took a sabbatical or a postdoctoral (I got a NIH special postdoctoral fellowship) and when I was going over and back. I said OK, I'm going to go by boat, it is going to be the last time I will ever be able to take the time to go by boat. I went across on the Ryndam, one of the Dutch all one class boats, and it took us 9 days to get over to Rotterdam. It was absolutely a delightful trip but it was a pleasure that I knew I would never be able to afford again; 9 days over and 9 days back, can you imagine giving up 18 days sitting on a boat in the ocean?

VM: I can imagine, because that was the boat we first came to America on...

SM: in '56.

VM: Where were you heading?

BT: I was going to the Eidgenössische Technische Hochschule in Zürich, working with Ruzicka's group. I took over a very simple project in which I wanted to...he was an expert in cholesterol and I wanted to study the radiation chemistry decomposition of it. I prepared very pure cholesterol and then attempted to learn methods of how to assay it for impurities that could be formed by radiation.

VM: Your saying "radiation decomposition" then reminds me of Dick Lemmon's work on choline chloride. That, presumably, stems from the same origin, does it? You were, both of you, interested in radiation decomposition of organic compounds

BT: That's right. And you see: what we found was that this compound, acetylcholine chloride, which we were making, very rapidly decomposed. Yet, if it was not labelled, it did not, it was chemically stable. Since the effect was so marked, we ran it down. In fact, I think I was on the initial publication on that because I was involved in that very closely. Then Dick proceeded to study what was going on and why and published a number of papers on that. Then, having done that work, we said if this compound can decompose by its own radiation at a very significant rate, then perhaps all these other compounds are decomposing. And so we went back and started to look at the rest of the compounds and realised that as we were making higher and higher specific activity materials, why they were self-decomposing at significant rates. In fact, by that time compounds that we had made two or three years before and they were beginning to look pretty sick.

VM: Before I go on the other compounds: did you look at ways of storing compounds so that you would minimise their radiation decomposition?

BT: Yes, of course, and many of the compounds were in sealed ampoules without any oxygen. I think that we even thought about trying to store them at very low temperatures. But that doesn't stop radiation decomposition.

VM: I seem to remember, but you will know better, that if you dissolve these compounds in alcohol, don't you tend to get an absorptive effect because some of the radiation hits the alcohol molecules and is therefore...

BT: Yes, that was one of the things that certainly was attempted. But when it hits the alcohol why it produces free radicals and those free radicals are reactive and can go back and attack the compound. The compound is more susceptible to radical attack than is the alcohol, you will, in fact, almost intensify the decomposition.

VM: We have talked a lot about the work you and your colleagues did with C^{14} , what about the other isotopes which were important at the time, first of all the radioactive ones of tritium and phosphorus, then there was work going on with stable isotopes. Were you involved in any of these other activities?

BT: Yes, I was greatly involved in the carbon-13 work but that occurred at a later date. We did not use much tritium in our work. We did use deuterium. There was a bunch of experiments that were going on with deuterium which Ann Hughes was running on

the toxicity of deuterium in whole animals, I don't know whether you have that in the record or not.

VM: I know about that because the first thing I did when I got to Calvin's lab. was work with Ozzie Holm-Hansen on deuterating algae. I was part of that scene. In fact Calvin suggested to me that was the way I cut my teeth on chromatography.

BT: Anyway, Ann Hughes found out that when you go above about 22-25% deuterium, why sterility resulted in the mice that she was working with. We did no C^{13} work and we had no mass spectrometrists in the group and we had enough questions that we could answer using the carbon-14 that it didn't become of interest. But in 1966-67 I went to Washington, DC for one year as a scientist in the Division of Biology and Medicine, and one of the things I took with me was a desire to develop carbon-13 as a tool. I began to push that back there and I found out that the Los Alamos National Laboratory was quite interested in carrying on a series of activities. And so I pushed through a programme of development of carbon-13 in which they set up a still for distilling...was it carbon monoxide? I think it was carbon monoxide, CO , to isolate the carbon-13. They became a major producer of C^{13} that still ended up, because of the oxygen-18 in it, at about 95.5% C^{13} . One of the reasons that I was pushing it was because the NMR techniques had been developed and they were obviously tremendously important because only C^{13} has an NMR spectrum. The carbon-12 and carbon-14 do not have an NMR signal. One enhanced the sensitivity of the NMR spectrum work and then provided a very interesting tool. So that there were then two tools to study carbon-13, both the NMR and the mass spectrometry work.

That programme then developed very strongly and went ahead and eventually commercial companies said "we're going to set up a still and make carbon-13" and you, the Atomic Energy Commission, can no longer make it because you can't compete with us. So, the laboratories were forced to stop it. By the way, that's the same thing that happened with respect to carbon-14 labelled compounds. Several places, but especially Oak Ridge, started to make labelled compounds and sell them on a commercial level. We didn't want to do it. That work continued and was very useful to the scientists of the United States until a number of commercial companies said "we want to make these labelled compounds" and they actually forced them (*Oak Ridge*) to stop selling carbon-14 labelled compounds, saying you can't compete with industry.

VM: The AEC couldn't compete with industry?

BT: Yes, that's right. It was not allowed.

VM: Why was it that the group chose not to become involved with tritium and phosphorus very much?

BT: There aren't as many good problems to be answered using phosphorus as there are with carbon-14. After all, phosphorus metabolism is relatively straightforward. John Lawrence used a lot of phosphorus but he used it because of its radioactivity and in the treatment of cancer and leukæmia patients. We did do some tritium experiments. It wasn't easy to detect. We didn't have good methods of assay for it and that probably was a big handicap.

VM: Then later on in the late fifties, I don't remember whether it was there before you left or not, there was some interest in using O^{18} as a tracer. Were you part of that?

BT: Yes, I was.

VM: There was a woman from Sweden, called Ingrid Fogelström-Fineman who did some neutron activation of oxygen-18 to make fluorine-18. Do you remember that?

BT: I do.

VM: I don't know whether you were part of that.

BT: I wasn't part of *that* experiment, but I was part of an earlier experiment because, being somewhat interested in the NMR work, I co-operated with a man by the name of Harry Weaver who was down at Stanford and at the Varian Laboratory, and we were the first ones who demonstrated the O^{17} signal by NMR in an organic compound. We took an organic compound and prepared it and put it into the NMR. O^{17} has a rather broad signal — it's not a very sharp signal like the carbon-13 signal is — and got a very nice band and published it. That was the first time that anyone had published something on O^{17} . Now O^{17} since then has become particularly from the work that came out of the distillation of the CO at Los Alamos much more available and both O^{17} and O^{18} are in their own rights very interesting isotopes. To the people who were working in photosynthesis these were very important. Remember, that in the photosynthesis work that the enzyme, ribulose biphosphate carboxylase oxygenase, is both an oxygen O_2 enzyme and a CO_2 enzyme.

VM: The last question I think we might discuss in this phase is: eventually you left.

BT: That's right.

VM: In the beginning everybody was very young, weren't they. Even Calvin when he started the group was in his early thirties, 34 or something like that. I guess you were all younger than he was.

BT: Oh, yes.

VM: You must have been in the late twenties — ish.

BT: Since I was born in 1921 I would have been 25.

VM: Well, there you are. That was the typical age of many of you at that time.

BT: That was the age of Pete and Jim and all the rest of us.

VM: And Andy was a little older, perhaps, but not very much. In the course of years of being employees in the place, you were also getting older. In 1957 you left and by that time you must have been — born in '21 — you were 36. Why did you leave?

BT: Sometimes I ask myself why I left because I was afflicted with a disease which is called "Berkeleyitis".

VM: Oh yes! It's well known.

BT: Anyone who has lived any length of time (*in Berkeley*) gets this disease and you recognise it. It has absolutely magnificent summers and winters and is an exciting

environment and why should you leave? I think I left for basically two reasons. One of them, I was seriously concerned about Melvin's health. I estimated, and at that time he had already had two heart attacks and there was good indication that he would have more, and I thought that there was a high probability that his life expectancy could be as few as few years. I had seen that in the laboratory when a really strong senior leader like this disappears that the group just eventually, although it may continue on for a few years, dries up and is terminated. I said (*to myself*): if I have an interesting opportunity. I probably should leave it this time.

That was one reason. I think there was another problem which was much more subtle. There was a fundamental conflict in the laboratory concerning the photosynthesis work. Basically, the photosynthesis work was the development of Andy and Melvin, and they worked closely together on it, in developing it. Of course, Al was an important assistant on it but he was really the junior member by far and away of this group. As it became apparent that this was a very important discovery and very important work, why Calvin eventually — and this was discussed rather openly between Gen and Melvin and I, and even Anne was part of some of the discussions — that there really wasn't room for both Melvin and Andy in the same group. They were both people of stature and they both wanted, and Melvin wanted all the credit and it was his group and he had started it and so he essentially said to Andy that he had to go. I think this did not sit well with me. I think that there was a little bit of reluctance on my part that I didn't like the situation. I felt a great deal of sympathy for Andy. As you know, he left and went to a very excellent position at Penn State, which he was not happy at. He had Hans Ostwald build him a very beautiful California-style house back there. But even that, in the middle of Pennsylvania, was not enough to make him comfortable. So eventually he left and went back to California which was his home.

VM: Did you also yourself hope for an academic position, did you see advantages of having an academic position in your own right, rather than (*be a member of a group*)?

BT: It's interesting. A number of people in the group, such as Heidelberger and Yankwich and all these people, had a very clear-cut idea when they joined the group that this was sort of a postdoc, experience, that they were going on as soon as they got a good academic position. In fact, they did, all of them. I did not. I did not have that level of ambition. I was not necessarily ambitious on that level. I wasn't bothered by the fact that I didn't have an academic position. In fact, when I joined academia and went here (*to Colorado*), and I came here as an associate professor without tenure (and two years to tenure), and saying "well, if I don't get tenure, I don't really care. It's not important to me necessarily whether I'm in academia or whether I'm in industrial research". I like research, I enjoyed research and would have been perfectly happy at that. So I continued here and so, of course, I got tenure and continued on here. But I have always felt that I could be just as happy in a place where I had adequate research funds to do interesting research and doing research.

VM: Had the atmosphere in some senses been different, and Melvin's health been different, you happy actually with the day-to-day, month-to-month, year- to-year environment?

BT: Oh yes. If that had been different, why I probably would not have left and given over my job to Dick Lemmon and would still be in Berkeley. Heavens knows who I would have married then what my children would be!!

VM: You might have married Anne because you met her, presumably, in Berkeley.

BT: Yes I did. I met her in '58. That's certainly possible.

VM: Looking back on it now, nearly 40 years later, regrets or no regrets?

BT: No regrets.

VM: You've had a happy life in Colorado?

BT: Absolutely. It's a delightful place to live. The climate is delightful.. The university has grown. During the period since I have come here this has changed from a...from not so...well, it was a good university, but it wasn't distinguished...to one of the very good universities. I'm pleased to say that I think I've had some part in it. I certainly was the one that pushed this group here in the Chemistry Department into accepting biochemistry as part of their regime. Of course, the department is now called the Department of Chemistry and Biochemistry. I can, in fact, remember the first real biochemist that we hired, was a guy by the name of Pete Albersheim, a PhD in biochemistry from CalTech, and I spent part of a year being a visiting professor in Argentina with a person by the name of Dr. Enrique Strachman (*spelling?*) who was an MD in Donner Laboratory who came from Buenos Aires. And I had been down and visited him and when I came back I said to the department that the one thing I wanted to do was to hire a biochemist and I want to be the chairman of the search committee for that biochemist. They said "OK". I was really almost surprised that they gave me the go-ahead. We hired this man Pete Albersheim, who is now a professor down in Alabama, and he was a very dynamic young man. From that start, the biochemistry division grew to the point where it is now a very famous and a very strong department. And it creates strength within the department with the diversity which one really needs in chemistry in the broad level.

VM: And you have also broadened out and have some industrial activity as well in chemistry?

BT: Yes, although that stuff started later.

(New tape)

VM: This is Bert Tolbert Tape 2.

One of the things I'd like to get your views on before we finish is the building. Many members and former members of the Calvin group have spoken in glowing emotional terms about ORL and the round building was, in a sense, an attempt to recreate the ORL philosophy in modern terms. What do you think? Do you think ORL was special?

BT: Yes: ORL was special, and just as many old buildings are, they are special. They are very nice to do research in because you can do whatever you want to them, you don't have to worry. They have wooden walls, you can nail through them, you can cut through them, they are easy to work in. ORL was also very interesting because it was one big room and the laboratory benches were put into there and small cubby holes were put around the periphery for the offices, as you remember. This meant that everybody was working in the same room together. They communicated then and used the common equipment. So, ORL was a very special laboratory in that respect. We didn't have it over in Donner. We had one big laboratory but then we had three or four small laboratories. And that splits people up.

VM: And you noticed the difference, did you, between Donner and ORL?

BT: Yes, that's right. The other thing, of course, that made ORL very special was that they were working, and were highly successful, on a very fundamental problem, namely, one of the photosynthetic pathways of carbon.

VM: And well focused.

BT: And well focused and directed. So that everybody's contribution you felt moved science forward in a significant way. Unfortunately, you succeed in making a synthetic compound in good yield, and so forth, then it goes to somebody who is going to do some major experiment. Although you have succeeded in making the compound, it isn't the fundamental discovery that you make if you discover a new ribulose biphosphate or something like that.

VM: As you know, the ORL came to an end, they demolished it, and another building was ultimately designed. I know it was after you left and we have already discussed that there was no discussion of this apparently before you left. But you have seen that round building and you know the idea on which it was based; what did you think of it? I don't know when you last saw it, and since Calvin retired from the directorship the whole character of the place has changed a lot, but presumably you saw it while he was still director.

BT: I have seen it at intervals every few years. I saw it in the early days, when Calvin was the dynamic force and it had the community effect that he wanted, and I have seen it since then when it has become basically a series of individual laboratories for professors in the Chemistry Department, which is what it is now.

VM: What's your view, what did you think of it really? The interesting thing is what did you think of it in the early days because if it gets degraded...?

BT: Absolutely delightful. The whole concept was delightful, in which essentially all the equipment and everything was community equipment. It accelerated the pace at which you could do research, you didn't have to go find a storeroom, to get a beaker or pipette, and there was always somebody around who could instruct you on how to use this counter, or something else. The communication was very good. I just think that it was great. But, it requires a strong director and it requires lots of funds to keep it going. What really killed this thing was that as funds became tighter there wasn't enough money to take care of the overhead. That was a major problem.

VM: Then, in addition, it lost its unitary character.

BT: It lost its unitary character because the people that were coming in were in themselves individuals who wanted to do their research and they didn't care whether the group corresponded to the others and they didn't care about the other research that was going on.

VM: It is difficult to see that type of building being built now, isn't it, a building for a man, as it were. Unless it's an institute; well, even if it's a dedicated institute, do these things really survive the first generation in the form and with the satisfactory character that they may originally have been designed for?

BT: I don't know. It all depends. If there is a proper succession done, a unified laboratory can do fairly well. But, not like Melvin's laboratory...

VM: ...as it's now become.

BT: Well, as it was, that does not survive. It might as well be a number of small buildings as that Round House design which Melvin put together. I guess the answer is "no". I don't think they really survive the one strong director. Unless a new director comes in which is strong enough and can fill the vacuum and can take it over. The other thing that I see that makes it very difficult today is the matter of funding.

VM: Even if a new director takes over an existing place, he has the problem of inheriting the people of the older regime and they may not be his choice of personnel unless you have an internal promotion to the leadership which may or may not work.

BT: Most of them do not work. I have not seen any of them work, so I will say that. That's not the way. There has to be another method of getting that level of creativity that you want.

VM: It may very well be that the people who do work as the regular staffers in a place like that are simply different people from the leadership people, the great man who's the founder.

BT: That's right.

VM: OK. Well, I think we ought to thank you very much for having talked so far. We're not finished yet because there's a mass of material for us to look at and, having looked at it, we may very well want to open up the discussion again for a bit.

BT: All right.

VM: But let's call a halt for this...

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name BERT MILLS TOLBERT

Date of birth JAN 15, 1921 Birthplace Twin Falls, Idaho
USA.

Father's full name ED TOLBERT

Occupation FARMER Birthplace Guthrie Center,
IOWA, USA.

Mother's full name Helen Estelle MILLS

Occupation Teacher/Housewife Birthplace MASON City, Iowa

Your spouse ANNE GRACE Zweifler

Occupation accountant. Birthplace BROOK, NY, NY USA

Your children ELIZABETH DAVID TOLBERT, MARGARET ANNE
TOLBERT, CAROLINE JOAN TOLBERT, SARAH HELEN TOLBERT

Where did you grow up? BOISE ID TWIN FALLS, Idaho

Present community BOULDER, CO

Education BS, Ph.D. Both in College of Chemistry
U. of CALIF, Berkeley CA.

Occupation(s) Retired, Former Professor of
Chem & Biochem

Areas of expertise Chemistry, Biochemistry, Nutrition,
Nuclear technology

Other interests or activities Horology, Business, Farm
Management.

Organizations in which you are active all of the above.

Chapter 28

ANNE G. (HARRIS) TOLBERT

Boulder, Colorado

July 17th, 1996

VM = Vivian Moses; AT = Anne Tolbert; SM = Sheila Moses

VM: This is talking to Anne Tolbert in Boulder on July 17th, 1996.

Anne, what was your history before you came to Calvin and how did it happen that you joined him as a graduate student?

AT: Well, I went to Brooklyn College and I had a professor named Dr. Statler who had some connection with Melvin, I'm not sure what the connection was, but he had gotten Murray Goodman his graduate appointment (*at Berkeley*). He was very proud of Murray because Murray had done so very well. When I was graduating from Brooklyn College he said "how would you like to go to graduate school?" I had never thought anything about going to graduate school. I thought I would go and be a high school chemistry teacher and stay in New York City for the rest of my life. But, somehow Dr. Statler talked me into saying how wonderful it would be if I went to California and worked with this wonderful professor. I don't know how I did it, but I got the courage to do it.

VM: Had you been to California before?

AT: Never. I don't remember ever having been to California; I don't remember ever going very far from New York City.

VM: How did you get in touch with Calvin?

AT: This is what I was saying. Dr. Statler had some connection, I don't know what this connection was, he is the one who placed Murray. When he had this student, me, and I did reasonably well although I wasn't particularly gifted really, but he thought "oh, I could get another student". So he wrote another letter (*to Calvin*) and said "I have another student, and Murray has done so well, would you like another student".

VM: So you had to do nothing. You just had to go there.

AT: I didn't even apply to graduate school. They just did this for me. It was wonderful. All I had to do was go.

VM: One day you got, presumably, on a train, and you went to California.

AT: I went to California. I actually car pooled with somebody and we drove across the country, and I went to California. Murray met me and helped me. He was kind of a mentor because he had been there two years. And he introduced me to the graduate secretary and introduced me to everybody in the laboratory. I knew nothing about what they were doing.

VM: You were going to be a graduate student in Chemistry. Did you meet the woman called Miss Kittredge? She was Lewis' secretary, I think.

AT: I met a woman, and I don't remember her name, who was the departmental secretary. Murray took me to see her and told me that she was the most important person that I would ever meet in the Chemistry Department and that I had better be nice to her. That was the person I met but I don't remember her name.

VM: That was Miss Kittredge.

SM: Can you tell me which year this was?

AT: I am pretty sure it was '51.

VM: So, when did you meet Melvin?

AT: Well, he was there. I don't have any memory of the day I met him. I kind of remember meeting Murray and the secretary. That was the big thing. I remember meeting Andy. I think I met them all before I met Melvin. I just arrived in the laboratory and found a room and just walked into this laboratory and they were showing me paper chromatography. I didn't even know what chromatography was! I was so green. I was so impressed because there were all these spots on the films. It was absolutely Greek to me. I didn't know what was going on. But, interestingly enough, they were such good people in that lab. that I soon caught on. I soon figured out what was going on.

VM: So you started working in the lab. straight-away. Did you take courses as well?

AT: I took courses but I didn't have a teaching assistantship so this was my pay.

VM: So you worked much of the time in the lab.

AT: The lab. work was my support. So I had to start right away working in the lab.

VM: In terms of nuts and bolts and deciding what you were going to do in the lab. and stuff like that: how did you work this out? Was it with Melvin or with Andy or the others? What happened? Did you choose, did they choose?

AT: I certainly didn't choose because I, as I said, I didn't have a clue — I really didn't have a clue. I can't remember if it was Melvin or Andy. I was definitely working for Andy, but I can't remember if it was Melvin or Andy who assigned this project to me. And the project I had was to work with Lorel (*Lorel Daus Kay*), we worked together. She was doing sedoheptulose degradation and I was doing ribulose. They were, at that time, assumed that when we degraded them there would be a 1:1 correlation of the five carbon atoms, 1:1. They thought it was just some simple kind of mechanism

where you just added two and took off two; I don't remember the story because I really don't remember it! But I remember we were going to come out with the same kind of results. That was assigned to me because they knew what they wanted done.

VM: Did you have any experience with sugar degradation?

AT: None. They were so nice. I don't know why I was so lucky to have been given this opportunity, it really was a waste.

VM: What were you going to do? Were you going to start with Lorel's technology and modify it in some way to deal with the ribulose, was that the idea?

AT: That was the idea, I think. Was Lorel using osazones because I don't remember, but I think that was it, that was the first thing. The idea was to make compounds that you could then break apart and then count those and then make another kind of degradation where you counted — there'd be other atoms and just kept adding and subtracting. Every time we would finish doing this, we weren't the same at all. That's what I remember. I didn't have much faith in what I was doing because I didn't know how to do it; I didn't think I knew how to do it. I didn't go in there with the great confidence that I was going to really do a wonderful job. I didn't know what I was doing.

So when these didn't come out, I thought, well I didn't do it right. I remember a staff meeting we had where we presented the data, my counts and her counts and...

VM: Was this one of the Friday morning seminars?

AT: It was one of the seminar sessions where we just had to present this, you know, and everybody was really kind of uptight because these weren't the same. There was not this 1:1 correlation. It was really hard for me to defend this but I said "well, I think I did it right"; "this is the way I did it" and I told everybody. As it turned out, it was, I think, correct, and it was a more complicated cycle than they thought. But it was interesting because I went in really cold, really cold...

VM: ...and knowing nothing about photosynthesis?

AT: ... nothing about photosynthesis, nothing about sugars. I don't think I knew much chemistry either because basically Brooklyn College was the City College of New York, a teaching college, and while I was there I had also taken education courses because I was going to be a high school chemistry teacher. So I wasn't very prepared to go in there and be a researcher. But, as I said, it turned out pretty interesting.

VM: In order to do this did you get your own ribulose? Did you run your own chromatograms, and so on?

AT: Andy would run these mixtures — he would do that — and they would run the paper chromatograms and I would take off the spots. From then on, it was mine.

VM: You had one of these little elution set-ups for eluting the material off the area of the paper which Andy had found for you?

AT: Yes. We just went on from there. They knew, they were ready, they had a project ready. I didn't think this up at all. They had it ready, they knew they wanted to degrade ribulose.

- VM: That's usually the case with graduate students who don't know enough about the thing to design their own project; they develop that later. Where did you work, in ORL itself?
- AT: I worked right in ORL...
- VM: In the big lab.?
- AT: ...in the big lab. Alex Wilson worked in that same lab. and Andy worked in that lab., and Bassham was in that lab., and...
- VM: ...Lorel too?
- AT: ...and Lorel. And Alice was the secretary...
- VM: Alice Holtham, yes?
- AT: ...and Marilyn was there; I think Marilyn was there. Maybe Marilyn wasn't; I don't remember.
- VM: I think Marilyn might have been in Donner.
- AT: She might have been but we saw a lot of her. Alice was the secretary right there (*in ORL*). I got to know all of those people. I was right there. As I told you the other night, there was this little room, a little shack behind ORL, and one of the degradations needed to be under high pressure and somebody told me what I was to do, turn this knob and this knob and watch this dial, they told me once and then I went back to do it. I fully thought the whole little shack and I were going to just blow right up because nobody was there. I was all alone back there, but somehow it all worked out and I did that little project.
- VM: Did Melvin come in a lot of the time and talk to you in detail about what you were doing? Because he was your thesis adviser formally, wasn't he?
- AT: He was formally my thesis adviser but my memories are that it was more Andy; I think it was more Andy.
- VM: Did everybody talk with everybody else in the room? Did everybody know what you were doing and you knew what they were doing?
- AT: Yes; well, they probably knew more what I was doing than I knew what they were doing. What I was saying was that there was a lot of communication, it wasn't secretive at all. It's just that if you ask me what everybody was doing, I couldn't tell you and that's probably because I just either don't remember or didn't understand at the time what all everybody was doing. I had a relatively simple project.
- VM: I wonder if this sort of thing happened: as you got data, you and Lorel since you were working close together, got data and were talking about it, whether everybody else joined in and tried to interpret it, all at the same time. Was it that close, that people were constantly aware of what was going on, or was it longer intervals than that when you made some sort of formal presentations?

- AT:** I think it was longer. I don't have a clear memory of people standing over my shoulder, trying to figure out what the count on that carbon was. I don't have that feeling, I don't remember that, but I remember presenting it at one of these seminars and having people really sit down and start thinking about what did this mean, did it mean that the cycle was different than they had previously thought.
- VM:** In the end, of course, they resolved this — were you there when they began to get the complicated schemes of the transketolase and transaldolase?
- AT:** I was there when they started working on that. As I said, I was there from '51-'53 and I think by '53 they had figured it out; they had figured out at least a pathway. I am not sure they had all the reactions; I don't remember.
- VM:** You were part of that discussion?
- AT:** As much as...yeah. It wasn't that people...people would listen, they didn't shut you out because you were a graduate student. If you had something to say, they would listen. It was a wonderful group. I had no feeling as if these people aren't treating me well in any way. I was just in awe of all of them. I thought they were all so wonderful and so smart.
- VM:** All the time you were there?
- AT:** Uh huh.
- VM:** Did you never feel entirely at ease with them professionally?
- AT:** No, never, never. No, I never did. When I came in with such a weak background and probably today a student would never get into a group of that calibre with my background.
- VM:** What about your relationship with the other graduate students? Were they, too, awe-inspiring?
- AT:** There weren't very many graduate students. My memory is not...unless it wasn't correct: in that group. I'm trying to think of who the graduate students were.
- VM:** Murray, of course, was a graduate student, and Alex Wilson was a graduate student. I don't remember who else were graduate students.
- AT:** I don't remember it. There weren't a lot of graduate students. It wasn't a heavy graduate student group. It was a group of professionals doing professional work with some graduate students in there. I always felt that the social life was wonderful, they had parties and they had...
- VM:** Wonderful in what sense. You say they had parties?
- AT:** What was so great about it? I was in awe of them that way, too, because here I came from New York, had never gone skiing, never gone to see mountains, never done any of these things that these people would do every weekend. They would have group ski parties or...
- VM:** And you would go?

AT: Yeah; everybody was invited, so I just went and did it?

SM: Did you learn to ski?

AT: No. I went up there but I never did learn to ski.

VM: Where did you live?

AT: I lived in a rooming house. I didn't have an apartment, just a room, just a room. I was, as I said, socially and intellectually very green.

VM: Well, I presume you learned.

AT: You learn a little, you learn a little. As time goes on, you do learn, yes.

VM: You were there for only two years in which time you wrote your PhD thesis?

AT: No, I never got a PhD. I only took a Masters degree.

VM: Was that your original intention?

AT: No. But after the first year, I married another graduate student. He got a job at Harvard so we left. So I took a Masters.

VM: Which field was he in?

AT: Physical chemistry. Nothing to do with Melvin.

VM: When you did this work with them, and published with them, which name did you publish under?

AT: I think by that time, because it was toward the end, I published under Harris which was my married name.

VM: Did you use the initial "Z"? I seem to remember.

AT: "Z" was my maiden name, Zweiffler.

VM: So you published under that name of Anne Z. Harris.

AT: That's right; I think I did.

VM: I haven't looked recently, but I remember your name was on at least one and, no doubt, more than one paper.

AT: Not too many but it was on that same topic. The only thing I did was that cycle working with the sugars, the degradation. That was the only thing I did.

VM: That was the subject of your master's dissertation?

AT: Yes, that was the subject of my master's dissertation.

VM: Do you remember now who was on your thesis committee?

AT: No.

VM: You didn't have any trouble with it, did you?

AT: No, none at all; it was so easy. It was so easy: when I think of what graduate students go through today and I think back how easy it all was, I think I should have gotten a PhD and gone on and did something. But I didn't. That's too bad.

VM: Now, of course, you're married to Bert Tolbert and indeed have been since 1958?

AT: 9.

VM: ...9, and did you know Bert at the time?

AT: Yes. We have talked about this. Once, only once, during that time and before I got married he came into the lab. one day and said "would you like to go for a ride in the mountains" and I said "sure" and we went for a ride in the mountains. That was the only social contact I ever had with Bert. I always thought of him as kind of one of the older guys.

VM: Did you know the people in Donner?

AT: No, not really.

VM: You didn't have any reason to go over there?

AT: Whatever was a group party, when they would do these ski things or these picnics or whatever, but I didn't work in Donner.

VM: And you didn't have any reason to go over there in the normal course of what you were doing?

AT: No, most of the stuff I did was with Andy in the Old Radiation Lab.

VM: How do you regard the time there? Do you look back on it fondly? Did you enjoy yourself?

AT: Yes, I think so, I did. I really think it was a time when I really learned so much about so much of the world that I had never ever thought about — just the countryside and seeing California and seeing ski resorts and seeing mountains. And then all these people! I'm trying to think. I think Melvin had won the Nobel Prize already by the time I got there. When did he get that?

VM: Oh no, it was '61.

AT: Oh '61. So he was working on it — I mean people were talking about it. He hadn't won the Nobel Prize. I knew there was something about him: he had had the heart attack. That's what he had: he had a very serious heart attack and I was always looking at him and wondering if he was going to die because Murray had told me that it was very, very serious, and that he wasn't going to make it. I knew there was something about Melvin that worried me or made especially (*anxious*). I was always a little bit afraid of him. I think I still am. I don't think I have ever talked to him comfortably in my life.

- SM:** 'You mentioned yesterday that you had a very good relationship with Gen. Tell us something about that.
- AT:** I just think Gen was such an open and wonderful person. I thought of her as a mother figure, I really did. She wasn't that much older than I was but she just was so caring and took care of people if they needed anything. She was just very wonderful. After I left Berkeley and when I came back the second time, that was when I was having marital problems, and Gen just jumped in and took care of me and was a mother to me. I have always loved Gen. Melvin was there, but he didn't have any people skills. I never knew if he wasn't interested or if he was just wrapped up in his chemistry. Gen was a simply wonderful woman.
- SM:** Right from the beginning you formed a friendship with her. How did that happen?
- AT:** I can't remember just exactly how I met her. She was so...whatever situation it was that she was doing with the group, I just felt like she was a terribly wonderful person. As I said, I got more friendly with Gen the second time when I came to Berkeley. That was when I met Bert, too.
- SM:** When was that?
- AT:** I left in '53, about '56, maybe, I came back. I'm not sure when I...
- VM:** You came back to do what?
- AT:** At that time, my husband got an instructorship at Berkeley so I came back.
- VM:** Were you working in Berkeley?
- AT:** No, and I didn't work. I really never worked in chemistry after I left...
- VM:** After your Master's.
- AT:** That's right. Really, as I said, that's why I know so little about what was really going on. I never did anything much with chemistry.
- VM:** What has your career been since then? Since it wasn't in chemistry, what was it?
- AT:** I didn't have very much of a career. Actually, I did work. I worked a couple of years in Boston at Massachusetts General Hospital for...can't remember the guy's name now...somebody who is now a professor out in Arizona I think. When I went back to Berkeley, I didn't work. I had my first child then and I just stayed home and was a mother. After I got divorced, I did go back to work for a little while and I worked for — I must ask Bert who I worked for because he knows and I don't — someone in the biochemistry department...
- VM:** in Berkeley
- AT:** ...in Berkeley, somebody who did B₁₂ or B₆ — B₆.
- VM:** Not Clint Ballou?
- AT:** No.

VM: Ed Snell?

AT: Snell.

VM: The Texan.

AT: Yes. I worked for Snell for a couple of years. During that time, Bert came back for a semester or summer or something — I don't know why he came back to Berkeley — and, because we had friends in common, we just met again and this time it took. So, when I married Bert and came back here to Boulder, then I never worked in chemistry again.

VM: But you did other things.

AT: I took care of two little girls and tried to have some more children and finally had two more little girls and took care of those, so we had four little girls. I didn't do very much until my youngest child was nine years old. I was 50 years old, at which time I started working here at the University of Colorado in Boulder doing clerical work and eventually became an accountant;.

VM: That's what you are doing now?

AT: That's what I'm doing now professionally. I'm an accountant for the University of Colorado.

SM: Earlier on when we were talking, you mentioned working together with Alex Wilson. Having met him. I am sure that many funny things must have happened with Alex around. Can you remember any stories?

AT: I really don't remember stories, although yes, he was a riot to be around. I do remember that often in the lab. things just don't go right. Other people would say just expletives, you know "oh darn" or whatever, but not Alex. Every time something went wrong he would come out with "life presents a dismal picture". I never knew what it meant, I didn't know the literary reference, but I have this memory of that. That's my memory of Alex. If anyone asks me what I remember of Alex, that's all I remember.

VM: One of the things that Alex told us was that when he was doing his big experiments he roped in everybody else in the lab. to help him. I don't know whether that was happening when you were there, or maybe you had gone already.

AT: I don't remember helping Alex at all.

VM: OK, so presumably...

AT: I either wasn't there...

VM: Well, I think we may have...You said that you had little to remember. You haven't had that little.

AT: Not very much.

VM: Maybe it's run out now. Thank you very much and it's there for posterity.

Regional Oral History Office
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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Anne Tolbert

Date of birth 7/10/30 Birthplace N.Y.C.

Father's full name DAVID ZWEIFLER

Occupation _____ Birthplace _____

Mother's full name GETTA STAHL ZWEIFLER

Occupation secretary Birthplace Poland

Your spouse BERT TOLBERT

Occupation CHEN PROF Birthplace TWIN FALLS, IDAHO

Your children ELIZABETH, MARGARET, CAROLINE, SARAH

Where did you grow up? N.Y.C.

Present community Boulder, CO

Education MS, MBA

Occupation(s) ACCOUNTANT

Areas of expertise _____

Other interests or activities _____

Organizations in which you are active _____

Chapter 29

NATHAN (NATE) E. TOLBERT

Okemos, Michigan

July 19th, 1996

VM = Vivian Moses; NT = Nate Tolbert; SM = Sheila Moses

VM: Here we are in...how do you pronounce it?

NT: "Oky-mos".

VM: ...Okemos in Michigan near East Lansing talking to Nate Tolbert on July 19th, 1996.

I notice from reading last night the chapter you gave me for *Advances in Plant Physiology* — that you were an undergraduate in Berkeley.

NT: That's right.

VM: In chemistry?

NT: In chemistry .

VM: Can you tell us how it happened and whom you knew there relevant to the Calvin and the photosynthesis story?

NT: Actually I did my, started my chemistry undergraduate work at Idaho and went to Berkeley in '37, at the time of the World's Fair on Treasure Island, and it was a great, impressive show for a farm boy from Idaho. I was a chem. major, primarily an organic chem. major with, as I said in that paper, a B+ complex and that meant that I never was good enough to really be the top dog and I was always struggling. But I took the regular courses in chemistry: I didn't know any of the professors personally very well. In that chapter I pointed out that when I was taking organic chem. lab. Ruben, who was doing photosynthesis research, the beginning of the carbon dioxide testing programme, was killed because he made phosgene in the sink and it got out into the lab. The other Assistant Professor, who had just arrived there the year before, was Calvin and he took over the photosynthesis project then. That was probably...I graduated in '41...

VM: You went there on a 4-year chemistry course, the regular undergraduate...?

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NT: The regular 4-year chemistry course. I graduated in May of '41 so that could have been the spring of '41 or it could have been in the '40s. I never knew Benson because he really wasn't there but his and Ruben's names are on their first paper.

VM: So you knew Ruben personally?

NT: No, I did not know Ruben personally and I also did not know Calvin at all. I knew the old teachers: F.C. Stewart for Organic Chemistry; my senior thesis was with Randall, a physical chemist. As I pointed out in that paper I was getting close to my future because I picked all of the papers on photoactivation of silver, which has a light phenomenon, and that got me a commission in the Air Force.

VM: But at that time you had no special biological interest, had you?

NT: No. In fact I didn't have any biological interest except that I was raised on a farm and any time you are raised around a botanical environment you then have a biological interest, I would say. But out of curiosity, I took the general course for medical students which medical students had to get a B in to get into Med. School. I took that course with no preparation, no idea about biology and pulled a C and I was crestfallen because I didn't know anything about biology let alone about biochemistry. So, as I said in the chapter, I was challenged so, I'll be darned, I went to Graduate School then in biochemistry. The general pecking order is math, chemistry and biology...

VM: I know; that's right.

NT: ...and I was sliding down the scale when I took that course in biochemistry. And my chemistry professor said, "Oh, no, don't do that!" They opposed that.

VM: So where was the biochemistry course given?

NT: Down in LSB, down in the Life Science Building — that's a beautiful old building. And you know, I don't think Arnon was there yet, I don't remember. I didn't know the botanists at that time.

VM: So you graduated in '41?

NT: I graduated in '41.

VM: And the next thing in your career was in the military?

NT: No, the next thing was I went to Davis. At Berkeley, in the Chemistry Department, when I graduated, it was automatic — everybody went down to Shell Oil Research Lab.; they sent all their students down to Shell Oil. They were really booming because the war was coming on and they were worried about oil substitutes and Calvin later got involved in that. We *had* to go there and that's where we *had* to have a job. They wouldn't recommend you anywhere else. I didn't want to do that. I took a job, instead, at Davis in enology and viticulture, actually. The guy in charge of their brandy research programme...

VM: That sounds a very attractive way of spending time!

NT: ...had just been called up and I took over his research programmes.

VM: Although you actually had no research experience at the time?

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NT: I had no research experience but he had a big programme going on the change in the pH with ageing and the change of the tannins and analysis of the tannins and he had a big cellar of brandy stored and made and I watched over the whole works. As I said in the chapter, I had been a teetotaller up to that time and had never tasted even beer. In the States we're pretty strict, you know, particularly in Idaho where I came from — that's Mormon country.

VM: And your parents were teetotallers as well?

NT: My parents were teetotallers and we had nothing but anti-Mormon jokes, actually, but we also were very strict in the sense that we didn't smoke and we didn't drink. We were Presbyterians.

VM: But I am pleased to see both you and your brother have graduated beyond that stage.

NT: We quickly graduated. I corrupted the rest of the family; my going into the wine business then brought them along. I helped establish many of the wineries. There was no Sonoma Valley then. We planted the Beaulieu Vineyard almost physically by hand down in Santa Cruz.

VM: Do you mean, when you say "we"...

NT: Myself...

VM: And colleagues from Davis?

NT: ...and colleagues in the Department of Enology, in this department. Up until almost the war, this country was also teetotal, during prohibition, and this had wiped out the California wine industry. There were no students, nobody with know-how, so we had to teach, we had big courses and I taught in that. We taught wine tasting; we used to go to France and bring back cases of wine, and to New York and bring back cases of their wild grape wine, and teach the students blindfolded how to taste for sulphite and acetate and where the wine, in general, came from.

VM: But not, presumably, when you were there because the war was on.

NT: I was there the first two years of the war; I was there '41 and '42.

VM: But you couldn't get to France at that time.

NT: You're right. I guess we had French wine; it was shipped in. No, we didn't go to France, that's right. Later on they did, I didn't.

VM: When you say that you recreated the vineyards, weren't these commercial vineyards?

NT: No, they were all wiped out. The only grape that was left after prohibition in California was Zinfandel. That was only by a few Italians who moonlighted and made bootleg wine. The Californians, at that time, '41, were bringing in, let's say, the Cabernets to be planted and most of them didn't know how to make wine, didn't know what grapes to select, or anything.

VM: So you spent a couple of years in Davis.

NT: I spent two years there and then I went into the Air Force — I got called up, so to speak.

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VM: How many years were you in the Air Force?

NT: Oh dear, from '42ish to the end of the war (be '45) — 3 years. This did not involve the Berkeley group at all, but my brother, who was one year underneath me in age, stayed right in the Rad. Lab. — in the Radiation Labs., so to speak — and took his Ph.D. degree under the Atomic Energy Commission's umbrella running the Rad. Lab. and he actually worked for his living up on the Hill at the beginning of that programme and we would go back there.

VM: So you had contact.

NT: So we had contact. And in fact, I went from San Francisco to the Philippines on the first big 4-engined plane that could fly the route. The point I want to make was that even then we would go back. And I had a sister who got her Ph.D. degree down in LSB in biochemistry and nutrition. So that I would be back in the lab. a lot. And actually, for some reason or another, I guess it was only Donner's Lab. that I remember at that time.

VM: Well, Calvin didn't have ORL.

NT: No.

VM: No, that came later.

NT: Bert was up on the Hill, that's right.

VM: Where were you actually based during the war years?

NT: All over, but actually overseas — do you want me to tell you everywhere?

VM: Well just...you weren't based in California?

NT: The first and biggest base I had was I was in charge of the photo labs. in the New England area. In other words, they had one big Air Force photo lab. I had about 25 people in it and we went out and took pictures of every airplane that crashed and we ran pictures of the photographic development of the CEO so his wife would have pictures and I got my next promotion quickly because of that operation and so forth. And then I went into photo intelligence where you look through that stereo thing and looked at...

VM: See whether it's done any damage.

NT: ...installations in the Netherlands and so forth and from that then I went to briefing pilots and navigators back in California at Muroc (*spelling?*) Air Force Base, which is still the experimental base where the shuttles land. From there, then, I went to the Philippines and in the Philippines I was in the headquarters group in an intelligence group. And our purpose was to be on the ground and get in and take pictures and evaluate things that were of interest to the Air Force, particularly before the GI got there. Once the GI got there, he took everything for souvenirs so we had to beat them in, which meant we had to go ahead of the troops oftentimes, which was dangerous and that's when I got shot because we were out in front of the occupation troops.

And then after the war, I was down recovering in Australia when the Japanese surrendered and then I flew almost directly to Japan and was in charge of picking up all Japanese aerial cameras before the GIs got there. In other words, again we took Japanese officers and transportation and went out to the Air Force fields before the

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GIs took them. This was exciting because we got terrifically welcome receptions and big parties and stuff like that. We took all these cameras and put them in big trucks and shipped them back to the United States. We were particularly interested to see whether the Germans had provided the Japanese with any research developments in aerial photography. They hadn't, that we could find. I have pictures of myself with great big aerial cameras that they had but they were very poor and had nothing we could see of value. But I wrote a big report and took lots of pictures of Tokyo right after the Americans got there and sent it to the *National Geographic*. They didn't quite accept it. They said they had other people that were writing and I suspect my writing was not so good in the end. But anyway, then I became lastly...I went and inspected Air Force bases as we were closing the bases, from Japan all the way back across Indonesia, Egypt, Morocco and so forth. I did that because that caused me to end up in Washington, DC. The name of the game at the end of the war for the American GI was to get back to the States. That took about a year for most of them but I got back in about 3 months by going through that inspection programme. Once you were in the states you just were issued out.

VM: So when did you leave the military?

NT: I don't really remember for sure. I think it was the spring or summer of '45 — when did the war end?

VM: August '45.

NT: Then I got out the next spring.

VM: And what did you do?

NT: I had already had a fellowship from Wisconsin that had been granted just as the war started, in biochemistry, and I simply wrote and told them I would like to come again if they would take me. And, of course, they took you because you had your own money.

VM: This was the GI Bill?

NT: GI Bill.

VM: So you went there the fall of '46?

NT: I went there, probably the summer of '46 and at that time Bob Burris, who was my major professor, had just moved from microbiology primarily because of the *Azotobacter* and the nitrogen-fixing bacteria. He had taken his degree in bacteriology and he had moved into biochemistry. It's complicated but anyway he became the plant biochemist of that department.

VM: Why did you choose to go there? I mean, when I ask that question it is done in the knowledge that Bert is your brother and Bert was at Berkeley and presumably you knew what was happening in Berkeley. What was in your mind — the thought of going back to Berkeley to do your graduate work?

NT: No, I had never thought about it because I thought I wanted to go into biochemistry. And at Berkeley at that time biochemistry was all down in LSB and there wasn't to my knowledge anything about plant biochemistry.

VM: You didn't know about Hassid?

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NT: No, I didn't know about Barker or Hassid, I really didn't...about the answer to that. You see, I had been in enology and viticulture. I was plant oriented from those two years. I didn't want to go back there and I didn't know about Barker and Hassid so what I wanted was a degree in plant biochemistry so I could come back into, say, viticulture or someplace.

VM: Did you not know — or maybe it was too early — but did you not know yet about Calvin's photosynthesis interest?

NT: No, I had no knowledge whatsoever about Calvin's photosynthesis. At the end of the war, it was at that time that they picked up on carbon-14. Bert has probably told you when he started working for Calvin; it was at about that time and they started their carbon-14 on anything that could be tested or they could use. Andy Benson had been a conscientious objector and had been up in the mountains, which becomes very important to me later because we used to hike the mountains. And he must have come back also at the same time that I went to Wisconsin.

VM: You were in touch with Bert, presumably, so you knew something of the developments going on.

NT: I knew but I don't have any major recollection of anything except that of his role in developing the carbon-14 programme.

VM: OK, so there you are in Wisconsin in the fall of 1946.

NT: Yep, like all graduate students.

VM: Right. What did you do? What was your thesis topic?

NT: The first thing I did was buy a canoe with three other graduate students and we'd row out into the middle of Lake Mendota and study German to pass our German test.

VM: You had to do that?

NT: Yes. And French. And so, research-wise, I started on polyphenol oxidase but I quickly switched after a while onto a new oxidase that Burris and another student, Carl Clagget had just discovered and they were saying, hey, there is another oxidase here. We don't know anything about it. We don't know what its function is. And in that era the phenomenon, we used to say, was a "terminal oxidase", these oxidases were terminal oxidases, that's the role of cytochrome-C oxidation. It's the end of an electron transport chain. And we used to consider the other oxidases, like glycollate oxidases and polyphenol oxidase and ascorbate oxidase as a system just to waste the hydrogens and transfer the oxygen without making ATP. And so that was the concept. That is an important concept for the future of my whole career because although that is partially correct for glycollate oxidase, it really wasn't that direct —take the hydrogens and transfer it to oxygen. Actually to regulate and control such a system you had very complicated metabolic pathways which prevented this from actually occurring.

Whereas many people later, particularly in England and in Canada, pursued the concept that you could oxidise glycollic acid to glyoxylate and turn right around and reduce the glyoxylate to glycollate and that became your terminal oxidase to waste energy. That's in my preparatory chapter in the *Annual Reviews*. As it turns out that has always been true except that it has a major function. It controls net photosynthesis and it controls the ratio of CO₂ and oxygen.

NT: No, we had no concert; we had no id

MM: And that was essentially what we were doing.

NT: That was my thesis. My thesis was titled “Chloro-

VM: So, you finished your thesis work in Wisconsin when—in about

NT: No, I finished it, actually, at the end of December, 1940.

VM: That was pretty quick.

NT: Three years — it was

NT: Three years — it was pretty quick. I was smart! No, I'm only kidding — I was lucky. I had a good enzyme, I could isolate it, purify it, look at the product. And Bert made carbon-14 labelled glycollates, he made C₁- and C₂-labelled glycollates; I could toss those into the Warburg flasks, look at the products and how much was oxidised and all of those things so we had two papers in JBC and some others fairly soon out of that.

NT: We had home made Geiger counters. We actually got the me-

NT: We had home-made Geiger counters. We actually got the models in part from Berkeley, from Calvin's lab., by golly, and Andy Benson was making these big Geiger counters. You probably remember those, with big windows. We made our own in Wisconsin after the Rad. Lab's. models. And, of course, Bert also was involved in this because he was counting carbon-14 all the time. So I had a lot of things going for me then with my contacts there through brother.

VM: So when you finished your Ph.D. at the end of '49 you were involved with plants, you were an expert in glycolic acid since that's what you had been working with, you had practical experience in the use of isotopes...

VBM: 100%

VM: ...and you knew what was going on by that time in the Berkeley lab. And then what did you do next?

NT: Well, this gets complicated. It was obvious because already Benson and Bassham were saying there was radioactive glycollate on the chromatograms. It was one of the early products they identified. It was obvious that I should go and work on that type of thing in Calvin's group. But in order to do it meant leaving behind in Wisconsin my fiancée and we never did get back together.

NOTE: All subjects are students

WM: In the same department?

NT No, she was not. I don't

NT: No, she was not. I don't know what she was in — she was in literature or something. We don't need to go into details. Her father was a major General Motors executive and he didn't think science was a worthy thing to be in. He thought you should be a GM executive. So between all the circumstances, I say, I went to Berkeley over their

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objections. Bert and I drove across country. My father had given me a graduation present of a new car from the farm in Idaho.

SM: This was 1950?

NT: 1950, yes. The car cost \$2,000 — a nice, new, red Chevy. And Bert came back by train and we drove it across country and had a wonderful time.

VM: But when you decided you wanted to go to Berkeley, was it your decision that you wanted to go?

NT: It was strictly my decision.

VM: And what did you do — you wrote Calvin or what? How did you decide to do it?

NT: Oh, gosh, I don't remember. I guess I may have, must have written and he said "Yes, come ahead." At the same time, Bert was already in charge of Calvin's administration in Donner and I don't remember who pulled strings but anyway they just said "Come."

VM: And they gave you a post-doc.

NT: They gave me a post-doc, the reason being that in January of 1950 phosphoglycerate was still not identified for sure. I think Bassham was on the verge of identifying that, and they had to do kinetic experiments to see whether the first product was phosphoglycerate or something else. I came from the Midwest where Gaffron was the big power at Chicago and we were just miles away from Chicago, and so we were really of the Gaffron camp. As you recall from those days, Gaffron thought it was some large molecular weight compound that would not move much from the origin of his paper chromatograms. From running chromatograms you also know that if you overload or have salts or anything in there the compounds don't move away from the origins, so that's where he went wrong.

VM: And you knew Gaffron personally?

NT: I knew Gaffron pretty well, yes. And Marty Gibbs knows him very well. You will find out that Marty Gibbs took care of, godfathered, Gaffron after retirement. And we also knew the Illinois group — Emerson and Rabinowitz. So that glycollate, then, was just one of the products being formed from CO_2 and we had no idea how fast or anything kinetic. Actually, Calvin was so interested in this that he had brought in six months earlier, before I got there, a post-doc. from Sweden by the name of Schou, Lisa Schou.

VM: She is now Lisa Wilkinson — she married Geoff Wilkinson who is a professor, retired now, a professor of organic chemistry who was at the Imperial College in London. We have already been in touch and had dinner with her last year and we're going to talk to her when we get back.

NT: Calvin had already taken her in just for glycollic acid and she published a paper with several of the people — Bassham and Benson and Calvin and herself and maybe Stepka or somebody else — as I was getting there or just shortly after I got there.

VM: So when you arrived in Berkeley you had not previously met Calvin?

NT: I can't answer that for sure. I may have met him when I went through Berkeley during the war. No, I did not know Calvin before then.

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- VM: Was this after he'd had his heart attack, after he had his big heart attack? I think that was '49.
- NT: I think it probably was after. He must have had that heart attack before I got there because when I was there in the '50s all he did was come into the office and then every afternoon around one or so he would lay on his couch and we would go in and talk to him.
- VM: While he was laying on his couch?
- NT: While he was laying on his couch. He was told to lie down and rest, and of course he didn't consider mental thinking as resting. So he would talk to us. However, I clearly remember him coming into the lab., and bouncing up and down and saying, "Let me see that chromatogram." And Al Bassham and Andy would get the chromatograms out and he would say, "Oh look, there's that spot!" — his typical enthusiasm and then he would go back to his office and think.
- VM: So when you got there, you were assigned space in ORL which was where the photosynthesis group was?
- NT: Yes, in the old building.
- VM: Had you had experience at that time of running chromatograms?
- NT: I had run chromatograms because in Burris's lab. we ran chromatograms, yes, but nothing at the intensity numbers that they did in the old Rad. Lab. — what did you call it, ORL?
- VM: It was called ORL, the Old Radiation Lab.
- NT: The Old Radiation Lab., yes, where they had a whole room full of chromatogram boxes. When I got to Michigan, here, the first thing I did was build a room like that.
- VM: I think all ex-Calvinists did that sort of thing.
- NT: And we just tore it out about two years ago, two or three years ago, and as soon as they tore it out I wanted to use it and I hadn't used it for ten years before.
- VM: When you got to there can you remember who were the other people at that time present in the lab.?
- NT: Well, let's say that in the lab. the people that I associated with the most was built around who went skiing and who went hiking and this were Al Bassham who would lead the trail singing "Come follow, follow me through the green woods, through the green woods", and Andy Benson, who loved to go rock climbing, and my brother and myself, that's four; we would generally go up in two cars and there would generally be eight of us. Clint Fuller came in about that time but he went some of the time but he wasn't so much an outdoor person. Bill Stepka — I don't remember him going with us on our mountain trips but he was also very energetic and, I think, a person that didn't accept anything unless he understood it. He was always critical of the thought processes; he would make you think. Dick Lemmon to some extent, but Dick was over in Donner making compounds but he also was an outdoors man.
- VM: Was Lisa Schou still there when you were there?

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NT: She was there when I arrived; I don't remember too much more about that. It caused Calvin to, before they had done kinetics of labelling, the rate of labelling, to feel that glycollate was not a first product. They published her paper actually a year or so after she left. I think she left about as I was coming there. But I remember her as a lovely-looking blond woman. Because they had already done the labelling and monitoring of this one spot on their chromatograms, that was glycollic acid, Calvin urged me to start on something else.

VM: He did urge you to start on something else?

NT: Yes, he said there wasn't anything more to do on glycollate. And I still have, but I couldn't find my file on all the projects he wanted me to work on, but I remember one of them — he was very interested in cyanide, why cyanide inhibited CO₂ fixation, because we knew that cyanide inhibited the cytochrome oxidase system and he was wondering whether it was cytochrome oxidase (*indecipherable*) cytochrome-C was involved in the electron transport so he was terribly interested in cyanide in addition. So I really did probably the first experiments — did almost 6 months on a whole stack of chromatograms with different algae, different cyanide levels and so forth and there was nothing to show from that. Nothing was done with the data because it didn't tell anything — it just inhibited CO₂ fixation — but that later developed into big stuff. Clint Fuller picked it up when I left and showed that the cyanide (he used radioactive cyanide), and showed that the cyanide was forming a compound which was a cyanhydrin complex with ribulose *bisphosphate*. So here was a new spot on the chromatogram labelled by cyanide which he knew was a cyanide complex with something and it turned out to be ribulose *bisphosphate*. And that led to the concept that the ribulose *bisphosphate* had a reactive carbon to activation that would bind CO₂, oxygen or cyanide and some other compounds. I picked that up in a Ph.D. thesis by the future director of Dow Chemical, sorry Dupont (he is now in charge of the Dupont European Programme). His name is John Pierce and John Pierce, for his thesis with me, reacted the ribulose *bisphosphate* with cyanide and then hydrolysed it to the acid CO₂ (COO) to form carboxyarabinitol *bisphosphate* and that compound binds to the enzyme (Rubisco) with a dissociation constant of 10⁻¹² — in other words it does not dissociate on the enzyme — and then we had the answer to why cyanide inhibited.

VM: Pierce was your student?

NT: Yes. So that the cyanide was inhibiting Rubisco by binding to the ribulose *bisphosphate* forming and being hydrolysed to this acid, carboxyarabinitol *bisphosphate*...

VM: Which just stayed there.

NT: ...then it could not dissociate on the enzyme. That probably was the compound that Fuller was working on; it was the reason we originally said cyanide was inhibiting and that compound was used all the '70s to study the enzyme. That compound would totally take Rubisco out of the picture and then you could isolate the enzyme and say how much there was and study its properties and most of the x-ray crystallography was done with the carboxyarabinitol bound into the enzyme because you could stabilise the enzyme.

VM: So when you were in Calvin's lab. you actually never worked on glycollic acid at all?

NT: No, I did not really work but I did work on it. We talked a lot about it, we followed it. Calvin, at that time, published a series of papers with chromatographic maps and on respiration (he called them "respiration") and on the products with different algae and

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glycollic was one of those and my name was on all of those papers because he put everybody's name on the papers.

VM: But you contributed to the work that was done.

NT: They used everybody's chromatograms. So I contributed chromatograms to those general papers at the beginning, that came out for the next year, actually, in '51 and '52.

VM: Did you also participate in the writing of the papers or the discussion that went on in the...?

NT: Certainly we discussed the papers and made the first drafts. Generally speaking the papers were published about a year later after he worked them over.

VM: I was making the point, asking about the point: was it usually the case that all the authors actually contributed to the discussion or did some of them just contribute experimental data for others to interpret?

NT: Well, that's a very big question and I would like to address it. First of all, the direct answer to that question: I have both in my CV. I may have my name on four of the papers, four of the *Paths*. I have both examples. Maybe a chromatogram or two from my file was used in the paper and we all discussed the stuff and you know Calvin had his research group meetings and seminar presentations so that certainly we all discussed them. And I think frankly — my impression is that Andy wrote the first drafts of those — that's my impression, and then Calvin polished them particularly to fit a scheme. And I don't know what the others will say about that but that's my feeling. Because I remember Andy working on papers because Calvin had to give a speech at the ACS meetings or some place and we worked up the first draft of the report which he then gave at the meetings and then those became a paper later. There was quite a long drag there.

VM: When Calvin went to these sorts of meetings, which he presumably did more than anyone else in the lab...

NT: He was the only one.

VM: ...did he consult you before? Did he keep you informed about his doings at meetings? Did he come back with information and things like that?

NT: I really think that Al and Fuller and Benson, who were there longer than I, could answer that better. But my impression is that he simply came into the lab. and said he had been invited to give a paper at a symposium at the next ACS meeting and didn't say it this way, but get to work and let's get a summary out on where we stand right now.

VM: And when he came back did he tell you what had happened or what else he heard?

NT: Yes, he came in, well the few times I was there, he would come to the group meeting and summarised what happened.

VM: So he was a window for you on what was going on, at least in some sorts of areas?

NT: As far as I was concerned. I never went to a meeting or anything while I was there although I had from Wisconsin because I was close to Chicago where many of the Chem. meetings were held. Now the other answer to this question — one is

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authorship which others may address — one of the standing thoughts were that he often listed the authors alphabetically. Because his name was 'C' and he only had to fight it out with Bassham and Benson and then Calvin, and Bill Stepka would be ticked off because he was clear down with me at the end of the alphabet! At this stage in life that didn't concern me at all, didn't care, but when you look back on it, and I don't mean to be negative on this, but because how else do you determine authorship? As far as I am concerned, when I later had my own groups, the guy who did the Ph.D. thesis, he was always senior author. But then you had the problem of how to throw in a lot of other people so I always took the position for myself, I was the last author and that is how most professors have done it; they become the last author. Because there are two places on a paper, the first author and the last author and all the rest are chickenfeed in between.

VM: That was actually the case on many of the Calvin papers by the time I got there.

NT: Later, that was true. He adapted that system and became the last author. So you should ask Stepka and Fuller about that.

VM: Yes, well I will, next week.

NT: You know, I don't know how to answer these questions because fifty years have gone by and your perspectives have changed. At that time there were just several things that counted. One was the weekend trips to the mountains; and your research. I didn't have any axe to grind or any thoughts about the future or anything.

VM: Well, you were a young man at the time, weren't you, making your way.

NT: Let me think, what...But at the same time we were a group of people who had had no previous contact so everything turned around the research. Your social life, what you thought about, what you did all week was strictly related to the research.

VM: Everybody in the group, not just your own bit.

NT: Not just your own although your own was what you knew best — it was the general problem of how was this thing worked out.

One other contribution, which isn't in my preparatory chapter, when I went to Oak Ridge, which wasn't until the last of '52, one of the things that Benson needed, because they still didn't know for sure how you got from the sugars over to ribulose biphosphate on the cycle, so we made carbon-14-labelled sedoheptulose, and large amounts of sedoheptulose as well, and sent them out to Berkeley and then they used that to work on the transketolase reactions.

VM: But you've moved ahead a bit, you're now in Oak Ridge.

NT: Yes, I've jumped ahead.

VM: What was the transition from Berkeley to Oak Ridge, your transition?

NT: My transition? Well, first of all, it's in the preparatory chapter which you may have read.

VM: Well, I read it very quickly but we would like to hear it from you.

NT: Basically, my career at Berkeley was cut short after only half a year because the Atomic Energy Commission at that time had only one man in charge of the biology

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programme for making grants. They had another group for the medical field and lots of people for radiation safety and so forth but they had only one biologist. He came to the lab. because Calvin was his biggest biology programme.

VM: Who was it?

NT: His name was Paul Pearson. He was from Utah. He wanted somebody to be his assistant that knew plants.

VM: In Washington?

NT: To go to Washington and be an administrator.

(Tape turned over)

This was mid-1950 so I had only been there a little over six months. And he came into the lab. and asked Calvin if there was somebody that would become his administrative assistant and all the people in the lab. were chemists, so they were disqualified, and they all wanted to stay in California — that was the name of the game; never leave Berkeley! They didn't want to go and that left me. I not only had my degree in plant biochemistry but I wasn't married to Berkeley so before I could say "Jack Robinson" (Calvin) said, "Well, Nate can go."

VM: Did you want to go?

NT: I hadn't had time to give it thought. I was right on the spot and I said, "Well, I guess maybe I could."

VM: But you knew that that was going to be an interruption in your research activities.

NT: No, I didn't think; I didn't even have time to think. *(Laughter)* You know how fast Calvin is, in his snap judgements. "Nate, you can do that."

VM: But what was your arrangement with Calvin? The post-doc. that he'd originally given you, was it a time-limited one or was it open-ended or what?

NT: I don't have any recollection of anything in writing on that.

VM: So did you expect, as far as you can remember, did you think you would go to Washington for a period and come back? What was in your mind?

NT: First of all, I didn't know, but we all were interested eventually in getting a faculty position so if I had thought I probably would say, "Well, look, I should be able to find a faculty position from Washington as easily as from Calvin's lab." That really isn't true because once you leave research it doesn't take you long to go down hill. Anyway I jumped in my little, old, red Chevy that I'd gotten the year before from my father and drove to Washington.

VM: And set up shop as Pearson's assistant?

NT: As his assistant in charge of...Primarily, he and I read all grant applications. There was no NSF; there was only the Office of Navy Research. NIH was not making grants to plants and the USDA never made grants either at that time. They just supported their own labs.

VM: So you were in the office from which Calvin got his money?

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NT: Yes, that's right. Now you could say, "Oh boy, he's pretty smart isn't he?"

VM: Yes!

NT: But that idea wasn't even discussed at the time." As it turned out we developed or had big plant science programmes at Brookhaven, which was a big photosynthesis group, at Oak Ridge where Arnold and other photosynthesizers worked, Argonne and Berkeley. And of course I got to go to all of those and talk to them about the research so it was quite a broadening experience.

VM: Don't give me any secrets or break confidentiality that even now you shouldn't talk about, but when the grant applications came in — I seem to remember them as being a very formalistic sort of thing that they turned in every year, a progress report and said what they were going to do next year and the money seemed to come.

NT: The money came.

VM: Was there much of a scrutiny in the office?

NT: Pearson and I read them and we were the sole decision makers and my recommendation Dr. Pearson could override it if he didn't want to, but basically the Director of the Biology and Medicine Division of the Atomic Energy Commission at that time was in Boston at the Children's Hospital. His name was Shields Warren. I used to drive him back and forth from the AEC Building downtown Washington out to the airport and had nice discussions with him and his attitude was that we will put all the money we can into biological research. Don't worry about overhead, don't worry about anything, just worry about getting more money because the biology programmes need to be supported. That attitude led in, while I was still there before I left Washington, to the beginning of the National Science Foundation.

VM: Incidentally, was the name of that man Shields Warren or Warren Shields; which way round?

NT: Shields Warren, Warren was his last name. He was a big power, along with the guy who founded the NSF. I used to complain, "Oh, look, these places are getting a big overhead," and he said, "Well, they need it. Universities have to build up their programmes. Don't you worry about the overhead". And that has been a good attitude that was followed in the States until recently. The last 20 years the overhead has become a debatable situation with Congress but in the beginning, even for NSF, the idea had been to get money into the universities to build up their research programmes. That was true in physics, chemistry and biochemistry.

VM: How long did you spend in Washington?

NT: I was there from the last of '50 to the last of '52 — two years.

VM: And earlier, before we started this conversation, you mentioned you were also responsible for funding Arnon's work in Berkeley? Or was that at a later stage?

NT: You know, I suspect Arnon had a grant. Yes, I think he had a grant from us then. I don't remember that detail.

VM: But you knew Arnon at the time or was it later that you got to know him.

NT: Let's say I don't know for sure. If you had a way to confirm it I would guess that when I went to Berkeley I went down to see Arnon.

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VM: When you went as a post-doc.?

NT: No, as an administrator.

VM: I see; yes.

NT: When I went as an administrator I also would look at all the AEC grants at Berkeley and Arnon had one probably by that time.

VM: That means that you were presumably aware of the lack of collaboration between the two big photosynthesis groups on the Berkeley campus.

NT: Oh, yes, yes. We all, in the lab. in the '50s, were well aware of that. As far as I am concerned, from the very beginning of the time when Arnon started. And, I don't know, Bob Buchanan might tell you when Arnon became really active. Probably in his Ph.D. thesis (Buchanan's).

The Gatlinburg Photosynthesis Symposiums in the '50s were the only photosynthesis symposiums and I ran those through Alexander Hollander, the Director of the Oak Ridge National Lab. Those were in '55, '56 and '58 — 3 Gatlinburg Photosynthesis Symposiums — of which I was sort of in charge of and even then they were writing the song I gave you last night to the tune of "Davey Crockett" about Arnon.

VM: Was the AEC concerned that two of its grant holders on the Berkeley campus were not communicating very well — or did they not regard it as their problem?

NT: No, they never were concerned. When I was there, there was no great concern, at least. We were just supporting (*both*). This was a wonderful time for science. When we got a good research programme in we either said, "Hey, this is great. This is from a top man. Support it." Or, if we had doubts we would send it to somebody that we knew knew the topic and ask them for their comments. But we made the decisions. We were basically supporting most things in photosynthesis. That was then radiation, light radiation.

To just finish up on this administrative...there's a lot of things involved...but historically my post-doc. in Oak Ridge was Bob Rabson.

VM: Yes. He was your post-doc.?

NT: He was my post-doc., period. He came from F.C. Steward, who was a friend of Fowden (*Editor: Leslie Fowden, a British plant biochemist, later Director of the Rothamsted Experimental Station*) — Fowden and F.C. Steward. This was an amino acid metabolism guy. He (*Rabson, presumably*) got his Ph.D. at Cornell with him (*Steward*) and came directly down and spent several years with me worrying about the glycine/serine story, where we worked it into the C₂ cycle, and then he took over the same programme for 30 years.

VM: Where is he now?

NT: He just retired and is still living in Washington, DC.

VM: You have...?

NT: I guess you don't want too far beyond this era, you would like to stay with...

VM: We would like to stay within the period roughly up to about 1960.

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NT: Let me first of all make the pitch that is in this paper that I gave you and that is that the Calvin cycle — actually Al Bassham preferred to call it the reductive photosynthetic carbon cycle — and we have simply tacked on to that now the C_3 reductive photosynthetic carbon cycle so we can trivialise it as the C_3 cycle. Now when it was realised from George Lorimer's and John Pierce's work with us and with Bill Obern at Illinois that the enzyme was also an oxygenase — that means that that enzyme was a dual catalysing two reactions and two cycles. And the other cycle was the one that made the glycollate; it actually made phosphoglycollate and glycollate but you (*indecipherable*). This had been called the "Warburg effect" in the '20s, oxygen inhibition; it had been called "oxygen exchange" by the Germans; it had been called, of course, "photorespiration" by the Americans and the British; and it had been called the "dark CO_2 burst" as the things unwound; it had been called the "glycollate pathway" by me, and so forth. So that really, what it boils down to is that there are two reactions that were visible which form two carbon cycles — the C_3 cycle which is labelled by carbon-14 and the C_2 cycle which is labelled by oxygen-18, so that photosynthetic carbon metabolism is two cycles (C_2 and C_3 cycles), and I would prefer abandoning all these other terms, all these titles of papers and so forth, and just say there are two carbon cycles, the C_2 and C_3 cycle, in which the gas exchanges are the opposite and so net photosynthesis is the sum of the C_2 cycles. OK. I need to get that onto your tape because that is the crucial thing that is now going to transpire. Just like the ability of the enzyme to remove CO_2 from the air was stopped, by the oxygen: that was called the "Warburg effect". In 1920 it began to be called that. Oxygen inhibited CO_2 fixation — you couldn't take all the CO_2 out of the air because the C_2 cycle takes over.

VM: Do these two cycles occupy the same metabolic space?

NT: Absolutely. The same enzymes.

VM: Literally the same enzymes?

NT: Literally the same enzymes.

VM: Not the same enzyme in two different places?

NT: In the chloroplast the same enzymes. They occupy the Rubisco and the regeneration of the ribulose biphosphate. You see, each cycle has to regenerate the ribulose biphosphate every time it wants to turn around. Now, the C_2 cycle was grossly underestimated. You can talk to your friends in Britain and they know that we began to say, "looky, half the photorespiration is half the rate of photosynthesis". Because all the work that we did in Calvin's era was done with carbon-14 which labelled the C_3 cycle but not the C_2 cycle until it went around the C_3 cycle and then billowed. But if you started with oxygen-18, and I have a whole section in here — you may have read it — if you start with oxygen-18, you label the C_2 cycle and you never would see the C_3 cycle so we have grossly underestimated the C_2 cycle and, when you really look at the kinetics of the whole thing, you find out they are about equal because that is the equilibrium level. It takes the same amount of oxygen that you get from 21% oxygen, water saturated with 21% oxygen, gives you K_M values for the oxygenase activity and it takes 1,500 ppm of CO_2 to saturate the carboxylase. So the two cycles are running in very major amounts, and balancing the oxygen and CO_2 . That's the new concept that I'm peddling and I think you will find that it will slowly take over but everybody — particularly all of my competitors: Zelitch, Olgren (*spelling?*), Butts in England, the Canadians — they all have been — and Marty Gibbs — they have all lived in the era of photorespiration, like me, and they all are retired so there is nobody but me who doesn't have the sense to quit and I want to get the record set straight.

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VM: OK.

NT: We were talking about the chromatograms. One of the biggest things is the identification of the radioactive spots on the chromatograms and how that was done (and you are an expert on that). You not only had to get it to co-chromatograph, you first of all had to co-chromatograph with two solvents, that was the main thing. And then you had to do some chemistry on it, convert it into another product or something to be sure that it was as predicted.

VM: Well, you had to have a certain amount of inspiration to decide what the likely candidates might be for the compounds you were interested in.

NT: That's right. You had to know the chromatogram. This was the amazing thing that all of us who worked in it, including you, we could...I've seen my graduate students' mouths drop open when showing me a chromatogram and I say, "Oh, there's malic, there's glycollate, there's serine and here's..." "How do you know that?" Well, I just know it.

VM: You live it!

NT: You live it. But you know it because you have identified it. And later when you come to identify a compound you have to do very rigorous chemistry on it. And that was developed by Benson. You must give Benson credit for that. Primarily I would say Benson was a chemist also and his biology came later, and he and Bassham really were the original people in developing that. And then you followed.

VM: Well, they were the backbone, Benson and Bassham were the backbone who kept the thing running. People like me came in but we came in and learned from them. True enough, we contributed our own bit but they were the ongoing things (!*people*) for a long period.

You said two years in Washington and then did you go to Oak Ridge after that?

NT: I went to Oak Ridge. It is all in this preparatory chapter but basically I could see right away where I wasn't going to get into a university from an administrative position without any teaching experience and not much research experience. So I had to wiggle out. I had two cards in my hand. One was that I was going to work with a guy named Sterling Hendricks, which was phytochrome, and I had a programme there. And the other one was the Director of the Biology Division at Oak Ridge, which was Alexander Hollander, and he had said, "Come on down and I'll let you be in charge of my plant biochemistry group". So I moved to Oak Ridge.

VM: And you were there for several years, weren't you?

NT: Yes. I was there from the last of '52; I was there six years to '58.

VM: Until you came up here to Michigan State.

NT: Until I came here.

VM: And you have been here, then, since '58?

NT: Since '58.

VM: That's a long time.

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NT: That's a long time. I came here with two other people as professors of biochemistry. One of them you should know — W.A. Wood, Willis Wood, the famous microbiologist.

VM: Of the Wood-Workman reaction?

NT: No. That Wood was Harland Wood from Ohio but he and Willis Wood are very good friends and I also was a good friend of Harland Wood (he died).

VM: I should know who Willis Wood is.

NT: Well, he was the Editor of *Methods of Enzymology* and he did some work with Nate Kaplan and...

VM: I don't think I have ever met him but I vaguely remember the...

NT: He is at the Salk Institute. Anyway, the three of us came here to begin a Biochemistry Department. I had to get out of Oak Ridge because I developed an extreme allergy to the pollens. Oak Ridge is a beautiful, beautiful place, covered with rhododendrons and redbud trees — it looks just like England in the spring — but they have an awful lot of pollen and it is more humid — much more humid than in England — in those Smoky Mountains and I developed such a severe pollen (*allergy*) there but I found if I went to a coastal region or came up this far north, I got away from whatever was bothering me.

VM: But you enjoyed your stay in Oak Ridge.

NT: Yes. That era in the '50s the national labs. were well supported and you could do full-time research, you could have post-docs., and you could teach if you wanted to and go into the university level. It was a good research environment but essentially since the '60s those places have gone down hill and the research programmes have been transferred to the universities — university research build-up — which is probably good.

VM: So by the time you came up here Bert had already left Berkeley.

NT: Yes. That is a very interesting and important topic for Calvin's group. All of those people in Berkeley, and you lived this too, the original group before you got there, Benson, Bert, Fuller, Stepka and so forth all wanted university positions. So did I. And we all had to leave because the Chemistry Department wasn't going to allow Calvin to give them faculty appointments. Naturally, first, most of them weren't top-notch chemists, they were biologists and secondly they didn't want Calvin to empire-build so they simply blocked anything and all he could do was keep them as post-docs. And just for the record, you know the only person who stayed there was Al Bassham. The group of biochemists who came in to start the Biochemistry Department brought in other people. They did give Al Bassham a joint appointment.

VM: Yes, much later.

NT: Much later. And that didn't pan out very well. And that again is just history.

VM: But Bert had left by the time you came here so what were your links with the Calvin group after you took up your appointment in Michigan?

NT: None. I had no links with the Calvin group except through personal research discussions. I would see Al at meetings.

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NT: We would be in touch in correspondence with Al. He, too, became primarily interested in stuff related to the C_2 cycle. Essentially the C_3 cycle was finished by the time you were through. The C_3 cycle was accepted, was called the "Calvin Cycle" and that was photosynthesis and that fitted in with 150 years of thinking that photosynthesis is CO_2 fixation and oxygen evolution. But actually the point that we are making now is that photosynthesis is also oxygen fixation and CO_2 evolution and that the two processes give you the net and give you the atmospheric bound so that the C_3 cycle *a la* Calvin was finished with your generation and then Al started really working on photorespiration. He had a very important and famous paper that said there are two pools of glycine. One was labelled quickly and one was labelled slowly, and nobody could understand it. We didn't doubt Al but we couldn't understand why should there be one pool of glycine that quickly labelled and one pool that very slowly labelled? The big pool is the slow label.

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NT: That's the C₂ pool because it goes from the chloroplast to the peroxisomes to the mitochondria where the glycine is converted to serine. That was so discouraging to Al not to be able to understand that and nobody would believe him. You get discouraged after ten years and nobody would invite you to a meeting or anything. But actually the quick-labelled pool has not been ever figured out until...I think I know what it is. I just published a paper this year on a chloroplast pathway, which is in this paper. In the chloroplast there is another pathway for glycollate metabolism which makes the extra ATP. It becomes the photosystem-I cyclic phosphorylation that oxidises glycollate to glyoxylate and on to glycine and that is very quick — very quick, instantaneous — you don't have to move to the other compartments. And that's probably...one of my ex-post-docs, who is a botany professor, is also looking at some of the other enzymes, and they are in the chloroplast, that would make the glycine. I just wish I had a lab. and could jump on this. I could solve it in two years.

NT: Yes, but you know there is nobody left. This is another Calvin story, really. Calvin created all these people that went out and kept the photosynthetic carbon metabolism going — people that associated with him, from Marty Gibbs to Clint Fuller — and those people from the '50s to 1990 who were photosynthetic carbon men. There is nobody now left. There is no Warburg, there is no chromatography room, there is nobody getting grants on photosynthetic carbon metabolism.

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NT: This is a big bitch and I think you should put it in your book; I have it in my (*indecipherable*). To do research you've got to do molecular biology, you've got to do mutant work, you've got to do the biochemistry and enzymology and you've got to do the carbon metabolism. And you've got to do all four and integrate them. Otherwise, the guy just looking at his DNA maps, he doesn't know what he is looking at.

VM: One last question I would like to ask you. It's about the building. People who worked in ORL appear to have a strong emotional and favourable memory of the place.

NT: They sure do.

VM: You do as well?

NT: Sure.

VM: What do you think was so good about it?

NT: That was, I think, was strictly comradeship. It was a crummy lab.; it was a dangerous lab. Let me stop on that one. It was so dangerous, it was radioactive, it was hot. It had phenol all over everywhere. We bathed in phenol and we always had a bottle of methanol around to wipe the phenol off our hands before we got burned. Today phenol is a toxic compound. You can't even have it in your lab.

VM: We used to smell it by the bucketful.

NT: We used to smell it and we used to take our chromatograms — we'd get phenol burns on our hands rather than lose a chromatogram. All of those things were overridden by the comradeship of the people who were there. They were tied together by a desire to solve this problem, this big, hot problem, in just one little group. You take research on the HIV virus. It is a big field but those people are spread all over the world. But this was one little group tied together.

VM: Have you ever come across anything like it?

NT: No, not really. In fact I have just the opposite experience. On developing the C₂ cycle over the last 40/45 years, we've all become... everybody in the field doesn't speak to anybody else. We became enemies; we became disrespectful. We just didn't have any comradeship. The C₃ cycle was developed by one group, one person, headed by Calvin, whereas the C₂ cycle has been developed by literally fifty scientists in all the countries, all disbelieving the other person. So, no.

VM: You are right about the comradeship and, of course, it extends to this day that the people now in their mature years in their 70s, many of them, retain this friendly feeling towards one another, very much so.

You agree, then, that the building had its merits. In time, as you know, it was pulled down and another one was built, and you have probably heard the stories about the philosophy that went into the round building that replaced it and you have been in that building. What do you think of that building? Or at least: I don't know whether you were in it at the time it was still a unitary building, when it was Calvin's.

NT: Yes, I was.

VM: What did you think of it as a modern version of something like that?

VM: A target.

NT: A target if you think about it. Al Bassham was the only guy there and he was struggling to understand the C_2 cycle. He didn't know about the oxygenase activity in Rubisco. He just was looking at product. So he was using almost an outdated procedure to try to understand the C_2 cycle where there were twenty of us other guys approaching the C_2 cycle by all of the other techniques. And so they were not a controlling force. To this day I doubt if Al really realises what he was working on. He will when he reads my chapter. He, alone, wasn't able to make it into the comradeship. He had nobody to be a comrade with.

NT: It was different, they didn't have people, Calvin wasn't very active — he was still

VM: That's quite true.

NT: ...and even though he and Bassham went out and lectured and lectured about regulating atmospheric CO₂ and the greenhouse effect, they never saw that it was the CO₂-oxygen ratio that counted.

VM: Well, it provided an opportunity to you to spot what it was.

NT: That's what I say in my chapter. "Thanks, Calvin, you gave up half the photosynthetic carbon metabolism for me to play with for the next fifty years."

VM: Well, I think that's a very nice place to leave the story.

NT: I am thinking...you know I didn't prepare for this at all. I do think you are asking about why did Calvin's group succeed so well? And besides, when I used the term "comradeship", I think it was also they played together, they went skiing together, at least when I was there they skied together, they hiked together.

VM: It was a community.

NT: It was a community. We would leave the lab. at 5:00 o'clock in the afternoon and be up to Donner Summit to throw, to pitch our sleeping bags down. And that also promoted comradeship and that ties in with previous remarks. There was also always a certain amount of the people in the lab. against Calvin. Not against Calvin but the people in the lab. hanging together and here's Calvin over here. Now I have experienced that so many times as a professor later on. The professor sits in his office

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and the poor slaves are out here in the lab. working away and they think everything they are doing is their work. It is their hands, their work and their ideas and if the professor gets an idea and says something to them they forget that and it is their idea. So that it is the traditional tug between the graduate students and post-docs. versus the professor and myself and, I think, for Fuller and Stepka and the rest of it, we had no concept of this. Basically we didn't dislike Calvin at all but we would say, "Oh, he's the king, he's the boss, he asks the only questions in the seminars and nobody else opened their mouth until after he emptied his mind of questions"; that kind of stuff. Well, that tradition is actually in the Herr Professor tradition of Germany and Europe, in the European labs. It is a slightly different form but it's still there, even in the American environment where you are supposed to be...everybody is an individual and everybody is free there still has to be a leader that ties things together.

VM: Well, I think one of the points may be for young people, and it was a very young group at the time when you were there, it was an advantage for these young individuals to have their names associated with Calvin in their publications. For awhile, anyway.

NT: For awhile.

VM: Before they wanted to become independent.

NT: Particularly then when he got the Nobel Prize. Yes, that was the tradition.

VM: Yes. And I think Al is a clear example of that. Al, in time, went completely independent and no longer published with Calvin but in the early days he did.

NT: Yeah. But there again, when I talk to Al sometimes about his data on the glycine and the serine and the amino acid stuff which Fowden was also working on, Calvin didn't even want to be associated with it. It wasn't that Al went independent, I think that Calvin said, "that's not important". Calvin said, "I only want to be involved in electron transport in the photochemistry", at least when I talked to him.

VM: As I saw Calvin in the later period that I was there, up to 1971, what tended to happen was that Calvin had a series of enthusiasms: photosynthesis was one of his earliest ones, of course. But there were the moon rocks enthusiasm, the origin of life, the planaria, the finding the (*cure for*) cancer, and I left off the cancer enthusiasm. Each new one of these things, he was always associated intimately with what went on at the beginning and wrote the papers and then, after a while, he would move on to something else and leave people in place so the lab., in a sense, was a compendium of Calvin's ideas, one after the other, each represented by a research leader.

NT: True, true.

VM: And these people, then, became essentially independent of Calvin, as you say partly because Calvin was no longer so interested and partly because they, themselves, had become older and more responsible.

NT: Yeah, I agree completely. I used to see Calvin at meetings and I saw him in Moscow and we had him here for a Dow Symposium and all that kind of stuff, and you are right. He moved on, so to speak. But, at least in the photosynthesis field, he got off into the artificial rubber game, which was just not viable.

VM: And the *Euphorbia* oil...

WILLIAM C. HILL

NOTE: In the last 6 days, 140 D. melanogaster flies were collected from the same area.

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NT: Right. We've full circled it.

VM: Full circled it.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Nathan Edward Tolbert
Date of birth 19 May 1919 Birthplace Twin Falls, Idaho, USA
Father's full name Edward Tolbert
Occupation Real estate/Farmer Birthplace Iowa/USA
Mother's full name Helen Mills Tolbert
Occupation School Teacher/housewife Birthplace Iowa/USA
Your spouse Eleanor D Tolbert
Occupation Teacher/housewife Birthplace Oxford, Mich, USA
Your children Helen, Carol, James, David

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Areas of expertise Photo synthetic Carbon Metabolism
Peroxisomes

Other interests or activities Same

Organizations in which you are active National Academy Science,
American Soc Plant Physiology

Chapter 30

MARTIN GIBBS

Woods Hole, Massachusetts

July 22nd, 1996

VM = Vivian Moses; MG = Martin Gibbs

VM: This is a conversation with Martin Gibbs in Woods Hole on the 22nd of July, 1996.

Of course, you were never a member of Calvin's lab. but you were aware of what was going on and were looking at it from the outside and it is in that context that I would like to talk to you, in the early days. So can we start by you telling me how you became involved in photosynthesis and what you were doing in that area around the late '40s and early '50s?

MG: In the late '40s I was still finishing my doctorate degree.

VM: Where did you do that?

MG: At the University of Illinois in 1947. And since post-doctoral experience was rare in those days, because the moneys were not there, I immediately took a job, and that was at Brookhaven National Laboratory. The other great event on the Illinois campus was that I met my wife there, over 50 years ago. And so if you look at that article I wrote for *Plant Physiology Newsletter* it indicates I went directly to the Brookhaven Laboratory from my degree from the Illinois campus. It is interesting that Bob Emerson came to the campus while I was there but I had already selected a major professor and therefore I couldn't work with Bob Emerson. I was finishing off my degree when Bob arrived. And so I left and went to the Brookhaven Laboratory.

At that time it was an old Army fort, or base, which was an induction centre for both World War I and World War II. So on arrival there were no laboratories, just barrack buildings, and no library. The nuclear reactor was being built. And one had to wait for about a year and a half before we had laboratories and so one could do no more than read the literature and, reading the literature, that is where I got acquainted with the research in the Calvin laboratory.

VM: What had been your background before that?

MG: I was a botanist. I took my degree in the Botany Department at the University of Illinois. I was trained as a more classical botanist and so when I came to the

Brookhaven Lab. (as) the plant scientist, I was *the* plant scientist, my job was to do service work for the mammalian physiologist on campus and what they wanted was radioactive sugar which they could use in their experiments. So they approached me and said, "There is a process called photosynthesis. Why don't you try and make us some radioactive sugars?" So my first assignment was to make radioactive sugar. I read the Calvin reports on what they were doing with higher plants and with algae. So I built an apparatus of my own and gave labelled CO₂ to higher leaves to isolate sucrose and hydrolyse to glucose and fructose. At the time I was doing this I think a paper came out with Greg Kracow (*spelling?*) and Zev Hassid and (H.A.) Barker in which they had taken *Canna indica* and had isolated radioactive sucrose, which was what I was doing at the same time. So after I made the sucrose and broke it into glucose and fructose and crystallised it and gave it to the mammalian people, I became a factory source. And so I gave radioactive sugars to I don't know how many people around the world, mostly in the United States.

Then I became interested in where this sugar was labelled. The only process at that time had been designed by Harland Wood in Cleveland. I went to Cleveland and Harland showed me the procedures and brought them back to Brookhaven. I decided to degrade the sugar and find out if it was uniformly labelled. As usual, as an isolated individual without any support, anything I had done was done by the Calvin lab. at least months before I even got ready for publication. I think the Calvin lab. did it by chemical means because Andy was such a superb chemist, and so was Melvin, and so they had chemically degraded the sugar, in pairs again, and so I did the bacterial procedure and I just confirmed what they had already done. So I just dropped out of photosynthesis completely; and so I got involved with the pathways in which sugars were broken down by plants and by bacteria because by then I had a very good supply, basically one of the world's supplies of labelled sugars — glucose labelled in the 1 position, glucose in the 2 position, glucose uniformly labelled — and so I decided to feed these sugars to microorganisms to determine whether or not the only pathway known then, which was the classical glycolytic Embden-Meyerhof pathway, was universal. It was a stroke of good luck that I C. Gunsalus came from the University of Illinois to spend the summer with me. Here was a very highly established investigator and I was still three or four years past my Ph.D. degree, and he said, "I want to use your sugars with my organisms." So Guny came for the summer and lo and behold he brought this organism called *Leuconostoc mesenteroides* and there we discovered a completely new pathway of sugar degradation. I gave a seminar a year later on the University of Pennsylvania campus and I met a person who was a boyhood hero to me and that was Meyerhof.

VM: Oh, was he there?

MG: Yes. When he left Germany he came to Pennsylvania. And he listened to the seminar (I don't think he believed a word I said). He believed there was only one pathway for the breakdown of all carbohydrates...

VM: His pathway.

MG: His pathway. And I tried to explain to him that the isotope indicated a different pathway. It didn't indicate the intermediates or the mechanism, it was just that it had to be a different pathway. That eventually came out in various laboratories. One of the major players was Seymour Cohen. (He just lives around the corner here now. He is retired and is over here.) Then, after I published that work, I got a phone call one day from Severo Ochoa saying he had a visitor from Munich, Feodor Lynen and he wanted to make some radioactive...at that time they worked on the condensing

enzymes, they called it, and they were looking at the mechanisms by which acetyl CoA combined with...gosh...

VM: With oxalacetate.

MG: with oxalacetate to make citrate. And they had labelled acetyl CoA. They decided to come to the laboratory. And they get here, these two very famous, established men, Ochoa and Lynen, to work with me to make their acetyl CoA. It was a very funny incident because I said to Ochoa and to Feodor, "Gentlemen, please put on gloves because we are working with radioactive materials." And they said, "Young man, we are established chemists. We will never get radioactivity on our hands." After about half an hour I said to them, "Now we just test the hands under the counters. Well, the hands just blew off and they just dived for the sink, and they were scrubbing away and scrubbing away, and finally Ochoa's hands were down to around 40,000 counts a minute and Feodor's down to 10,000, and Feodor said, "You know, Severo, it is because of your hot Spanish blood and my cold Prussian blood that you are running 40 and I am only running 10!" (Laughter) After a few days we made the compound and so off they went.

VM: All your syntheses were enzymatic syntheses, you were not a chemist?

MG: I was not a chemist at all. And so after I did the work with *Leuconostoc*, I went to the 1954 Botanical Congress in Paris; Feodor Lynen asked me to come to his Institute and give a seminar, which I did in German. At the end of this lecture in German a young grad. student said, "May we have the questions and answers in English, please?" But in that room was Otto Kandler. He came to listen to the seminar. And he and his wife invited me to their apartment for lunch and during the lunch they approached me with respect of coming to the Brookhaven Lab. because they said "We would like to take your method of degrading sugars, where you get individual carbons instead of pairs of carbons", and because Otto became interested in photosynthesis at that time. So they came with the express purpose of feeding labelled CO₂ to algae for short periods of time to make use of the *Leuconostoc* procedure to find out how the sugars were labelled.

VM: They already knew what Calvin's group was doing or had done?

MG: I assume they did. I didn't know that. So that was my introduction to photosynthesis. It came because of Otto and the *Leuconostoc* procedure.

VM: And that was in about '55?

MG: In '55 when Otto came with Traudl and their child Maya, the three of them. And Otto spent a half a year and then they moved on to Berkeley.

VM: Do you remember why they moved on to Berkeley?

MG: I just assumed it was pre-arranged.

VM: Because the story that I heard, and see if it rings any bells, was that Otto was on a Rockefeller grant, as I remember...

MG: I think that's right.

- VM: And the Rockefeller man, whose name I vaguely remember as Pomerat, but I'm not sure, actually asked him to go to Berkeley and he was reluctant but that's what I remember. Do you remember that?
- MG: No, I just took it for granted that they would spend half the time with me and half the time in Berkeley. And so they bought an old car and Otto had never had a driver's license so she did all the driving from the East Coast to the West Coast with their little daughter, Maya. I just took it for granted this had been pre-arranged. I was never consulted, no.
- VM: At that time had you...were there any discrepancies in your and Otto's degradation data and that of Calvin's? Because when Otto got to Berkeley there were furious debates between him and Melvin.
- MG: As I remember it, the only degradations that were carried out in the Berkeley lab. were done where they got pairs of carbons, that is carbons 1 and 6, 2 and 5, and 3 and 4; and they then concluded that the radioactivity in the two halves were equal: $1 = 6$, $2 = 5$, $3 = 4$. And the *Leuconostoc* procedure showed that was incorrect, as you well know. The 4 was labelled the highest, then the 3, and so on and so on. That was published and then after that was published Otto just took off and went to Berkeley.
- VM: Because Calvin would no doubt have seen that. That was really a tautomeric argument because if they measured it in pairs then it was an assumption to assume that 1 was equal to 6. I don't remember the details but ,if you are correct, then they were simply making that assumption and hadn't demonstrated it.
- MG: Part of it came because Andy had worked out, I recall, maybe it was Al, an elegant procedure for the breakdown of the sedoheptulose and in fact when we saw the sedoheptulose data and the ribulose data it was clear to us that the *Leuconostoc* data were correct. Because, if you remember the sedoheptulose data, there was a hole in the middle of the labelling and that was explained very beautifully by what we saw in the sugar, in the glucose. But because of the degradation procedures they had used, they couldn't take advantage of that and so we saw the ribulose data where the 1 was more than the 2 and the 3 was far more active than the 4 and 5. The interesting data was sedoheptulose. The glucose fit in perfectly with their own data.
- VM: Were you in touch with them at that point?
- MG: No, I had never met Calvin at that point, I'd never met Andy and I didn't know Al and I never in my life had been there.
- VM: When did you begin to know them?
- MG: Probably not until the '60s.
- VM: Oh, really; as late as that?
- MG: I was an isolated individual working by myself on the East Coast. I didn't travel very much, you see. I don't think I met Melvin until at least into the '60s and I don't recall meeting Andy until that time, or Ed Tolbert or any of these people. Because outside of the work I did with *Leuconostoc* and the degradation of sugars with Otto, I went back to plant science, plant respiration which was what I was doing. So I really backed off of photosynthesis.

VM: So you didn't have any great interest in photosynthesis at that time?

MG: No. It was a means to an end with the *Leuconostoc*. And also because people like Harry Beevers came to the laboratory, B.L. Horecker came to the laboratory and we put to use the *Leuconostoc* because at that time Bernie Horecker was involved in the conversion of pentose phosphates to hexose phosphates. And so we were synthesising labelled ribose 5-phosphate, giving it to liver extracts as well as plant extracts, crystallising out the glucose 6-phosphate, degrading the sugar to show the mechanism both in liver and in roots and in leaves.

VM: I'm trying to remember when, this would have been in the mid-'50s?

MG: In the mid-'50s.

VM: Because that's when Horecker and Racker were identifying the enzyme for the pentose phosphate cycle.

MG: That's exactly right. And so Horecker came and spent a summer as a means of confirming what he and Ef Racker had written down as to be the intermediate pathways.

VM: And, of course, their information was entirely relevant to Calvin and, indeed, they used it in Berkeley as leads to what might be happening...

MG: That's correct. I don't think Horecker and Racker looked at it that way. I think they were interested in mammalian tissue and it just happened to fit in with what Melvin was doing.

VM: Oh absolutely. But I think the fact that they discovered those enzymes gave him support for the mechanisms that he was playing with.

MG: I think there was no doubt. Unfortunately, I felt neither gave enough credit to each other. That is, I don't think Horecker and Racker really credited Calvin for what he had done and vice versa. I think they had gone on their own ways and never communicated.

VM: As you saw it from outside these groups, but aware of them, were you conscious of a lot of competition between them?

MG: No.

VM: They just ignored one another?

MG: I can't answer whether they ignored one another. They certainly didn't have much communication simply because, remember, Ef Racker was an MD who came through the field of psychiatry.

VM: I didn't know that.

MG: In fact, he came to our home. He was a very professional artist and when Ef came to our home in Ithaca, New York, he did portraits of our children which hang on the wall in our home. And Bernie Horecker, again, was in the NIH and his interest was in, really, liver and Bernie had never used leaves and roots until he came to Brookhaven. So I just don't think they communicated much with each other. I think they were

aware of each other, naturally, but I don't think they even went to the same meetings. I don't think Melvin came very often to the biochemistry meetings and, if he did, I don't think they were friends.

VM: And you were in very much the biochemistry circuit and the plant physiology circuit at the time?

MG: Yes.

VM: How long was it before there were other people in Brookhaven working alongside you?

MG: When I left Brookhaven I was replaced by Clint Fuller.

VM: When was that?

MG: In 1956.

VM: Were you still alone in Brookhaven?

MG: Yes. I was still alone.

VM: Not even a technician?

MG: Oh, I had a technician and I had an occasional post-doc. who would come but most of the folks who worked with me came in the summer.

VM: What did Brookhaven see as the purpose of employing you? To service the mammalian people?

MG: In the beginning, to service the mammalian people just to make labelled compounds for these people.

VM: Where did you go when you left Brookhaven?

MG: I went to the Department of Biochemistry at Cornell University, in the College of Agriculture, and I left Brookhaven because they didn't grant me tenure so I just left and became a tenured professor at Cornell University. I stayed there ten years before I came to Brandeis University.

VM: Where you then stayed then for the rest of your...

MG: For the rest of my career.

VM: ...professional life.

MG: That's right. So I didn't move very much.

VM: But you did become involved in photosynthesis in various ways later in your career?

MG: I became involved later but mostly at the level of the organelle after that, just to determine if the sugar is made by the same pathway as the intact plant using the *Leuconostoc* procedure and then I got involved in the regulation of the pathway, mostly at the level of the chloroplast and got involved in (*what I*) call "chloroplast

respiration". Chloroplasts do respire and they break down sugar by the alternate pathway and by Embden-Meyerhof to some extent. I was really never that deeply involved in what you call photosynthesis. Clearly if I had started with Bob Emerson, probably I would have been. But I didn't come from that background.

VM: But from your background, as you look at what went on in the '60s, how do you view the work of Calvin and his group in contributing to that resolution?

MG: Well, clearly it was the key work; it was the basic research of that period and all of us were just followers because they were the leaders and we just followed along. All we could do, really, was to fill in the little holes that fell through the network out there. I always envied them — this huge group of obviously very competent people — and clearly working alone as an isolated individual trying to compete with them was impossible.

VM: Did it ever occur to you to join them?

MG: No, I'm an Easterner. I am a Philadelphian by birth and I have made my whole life here on the East Coast. I was never invited.

VM: Many people more or less invited themselves and were more or less accepted. There was a time when the group was expanding quite quickly and Melvin would bring in good people who showed an interest. I can't predict what might have happened 40-50 years ago but it's not out of the question that had you expressed an interest you would have gone.

MG: Well, but I didn't know these people, you see, I had never met any of them. We might have attended common meetings but I didn't know them so we were never on speaking terms at all. So as absolute strangers it was difficult to pen a letter saying, "I am interested in what you are doing, would you mind having me in your laboratory?"

VM: I was able to do that as a post-doc. but then you were beyond that when...

MG: I was beyond that. I didn't have tenure at Brookhaven but I had a laboratory array. And I really wasn't interested that much in photosynthesis *per se*.

VM: So your connection, your only connection for a long time, was Otto going from you to them?

MG: That's about it. When Otto left I went back to working on plant respiration in intact plants and extracts, on the pathways of sugar breakdown. So when Otto left my deep interest in photosynthesis went.

VM: Well, I am sure we'll pick up Otto's story, at least I hope we'll pick up Otto's story next year when we get to see him. Because when he arrived in Berkeley, as I remember it, there ensued months of vigorous exchange of views with Melvin, to put it mildly. It led almost to a crisis of discussion in Melvin's lab. because there was a conflict of interpretation of data and there were a couple of big internal meetings to try and resolve it and collectively everybody agreed that he was essentially right. I don't remember now what Otto felt at the time but perhaps Otto will remember that.

MG: When Otto left Berkeley he came to Cornell University where Karen (*Martin Gibbs' wife*) and I were now living and spent a few days and he told me about this

compound, hamamelonic acid which he felt was the critical intermediate, and that's about all.

VM: He didn't discuss the big arguments?

MG: No, Otto never discussed the arguments that went on in the Berkeley lab. Well, Traudl occasionally came into the lab. to do the degradations and she also volunteered to do the washing of the dishes since I didn't have a dishwasher. I washed dishes as well as anyone else in the lab. did. She was very helpful in that respect.

VM: Was she a scientist?

MG: I really don't know her educational background but she seemed to be competent because the things we were doing were rather routine.

VM: Once you worked it out ...

MG: Once you worked it out it was a routine matter. She was just as good as I was at doing these routine things. She was there quite often once she could get a baby-sitter. In those days you couldn't bring a child into the laboratories of the Department of Energy so from time to time when she had get a baby-sitter she came in and worked alongside of him. It is a pity, I have some great pictures of that period, some big ones of that sort showing Traudl in the lab. and Otto in the lab. and so forth. They aren't here they are somewhere in Lexington, I suspect.

VM: Well, sometime, if you can be bothered to make a decent copy of one of them which shows you with them that might be interesting to have.

MG: I think we have one on a slide which shows us at a big lobster party because we always enjoyed lobster and champagne parties in the lab. and in those times 10 and 15 lb. lobsters were rather inexpensive — they were like a trash fish at that period — so we would make the lobster in the autoclave...(indecipherable)...a pot that large for a 10 or 15 lb. lobster. So we had parties and I think I have a picture on a slide of the whole lab. with Otto and Traudl and myself standing along this huge lobster after it was cooked in the autoclave. I don't think it has been made into a positive.

VM: What about one in the lab. — do you have one of them working in the lab.?

MG: Individually, but not together. I'm pretty sure it's individual.

VM: We have an odd picture of Otto, not of Traudl, of Otto in Berkeley at one stage. We are taking pictures of everybody now. I don't really suppose we can do a then and now in anything we publish but anyhow it is interesting to see what people looked like then.

MG: Otto then, as he is now, was a very intensive man and he was very devoted and loyal to the subject. He was essentially a 24-hour worker because he was there only six months and he accomplished an awful lot in that six-month period, and Traudl was very devoted to Otto and came to the laboratory, as I said, to assist him when he felt he needed the help. But then after they returned from Berkeley to Cornell when we were there, and he went back to Germany, I didn't see Otto for many years. We've had contact but very limited contact in the last 30 years or so.

- VM: Well, the last time I saw him was on a bus in Moscow in 1968, I think — that's nearly 30 years ago — at a microbiology conference or something of the sort.
- MG: Yes, Otto, you see, switched, as you well know, out of plant science and so we didn't go to common meetings after that. And I've been in contact mostly with Erwin Lanscow who...and also I had other post-doctorals from Otto's lab. that came to me.
- VM: So there was some sort of contact.
- MG: Again they came basically to learn the *Leuconostoc* technique and to also apply the sugars to microorganisms to see if the pathways were there. And we had just worked out the procedure for what became the Doudoroff-Entner pathway because we gave labelled sugar to an organism, a pseudomonad, which showed that the *Leuconostoc* pathway, the Embden-Meyerhof was not present and we had just done the isotopic work and within a year Doudoroff and Entner came out with the pathway.
- VM: I had realised you'd been involved but I hadn't ever heard your story put together in a coherent fashion to realise how it had started and why you became.....
- MG: We were 3,000 miles away and, as I said, I had not even letter contact with these people or telephonic communication with these folks on the West Coast.
- VM: Things are very different now from the way they were then.
- MG: That's right. Travel money was not as abundant as it is now and the annual meeting was the Federation Meeting, which was in Atlantic City, and therefore a local meeting for me, basically, so that I had never been to the West Coast in all that time. I guess I haven't seen Melvin Calvin, even now, in 20 years. I see Andy to some extent at the Academy meetings and haven't seen Al Bassham in 10-15 years. Ed Tolbert I see at the Academy meetings annually. The other people like Bert Tolbert or other folks in the lab. I don't know.
- VM: I am sure they would be delighted to see you if you were in the same place and looked them up.
- MG: Well, I did see...the last time in Berkeley I spent the day with Dan Arnon and with Bob Buchanan. That's right, I was on the Visiting Committee so I did see Melvin at that time — the Department of Energy Committee to assess the laboratory.
- VM: That's when you went into the round building.
- MG: That was the first time I'd been inside that building — that's the only time I was inside that building. And Al was kind enough to show me about the building and show me how the thing was cut up into wedge-shaped pieces of a pie and I had never seen the laboratory. I had never been invited to see the laboratory either, by the way.
- VM: There was an older building.. and they probably told you at the time that the philosophy, if that is what one can call it, for designing this was derived from the old wooden building in which the early work was actually done, which by chance, was a very open building, one in which the group had grown up, and Andy has nice stories about how he organised that building for them. It was a building that E.O. Lawrence originally had his 37" cyclotron in and when that went out it left a building with a big hole in the middle and they just took that space.

MG: I would suggest you visit Allan Brown, who was a very close associate of Hans Gaffron, and he was actively competing against Melvin and Andy in that period for the first labelled compound in photosynthesis.

VM: Which department was he or is he in?

MG: I think at that time he was in the Department of Botany at the University of Pennsylvania but when he did the work with Hans Gaffron it was done at the University of Chicago.

VM: OK. But I could presumably reach him through...

MG: At the University of Pennsylvania, Botany Department. Because he can fill in that period in the late '40s and that was hotly contested: what was the first labelled product of photosynthesis? Melvin obviously was pushing PGA; at that time I don't think Allan Brown and Hans Gaffron were. I think that's a critical period. I don't think I would depend entirely on information from the Calvin laboratory. Sam Aronoff was associated with that lab. and I know Sam is a very even-handed man but he was in that laboratory.

VM: OK. We'll do that when the opportunity arises. Meanwhile, many thanks; nice to see you again after so many years.

MG: You're welcome.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name MARTIN GIBBS

Date of birth 11/11/22 Birthplace U.S.A.

Father's full name SAMUEL GIBBS

Occupation MERCHANT Birthplace U.S.A.

Mother's full name ROSE SUGARMAN

Occupation _____ Birthplace U.S.A.

Your spouse S. MAREN

Occupation _____ Birthplace U.S.A.

Your children STEVEN JOSEPH JANET HELENE LAURA JEAN
MICHAEL IELAND ROBERT KVALE

Where did you grow up? PHILADELPHIA

Present community LEXINGTON MA

Education B.S. (chem. & ph.) Philadelphia College of Pharmacy & Science
Ph.D. (botany) University of Illinois

Occupation(s) SCIENTIST, EDUCATOR

Areas of expertise PLANT PHYSIOLOGY/BIOCHEMISTRY

Other interests or activities _____

Organizations in which you are active NATIONAL ACADEMY OF SCIENCE US
ACADEMY OF SCIENCES FRANCE ; AMERICAN SOCIETY OF PLANT PHYSIOLOGISTS

Chapter 31

RICHARD A. GOLDSBY

Amherst, Massachusetts

July 24th, 1996

VM = Vivian Moses; RG = Richard Goldsby; SM = Sheila Moses

VM: We're talking to Dick Goldsby on the 24th of July, 1996 in Amherst, Massachusetts.

Dick, can we start by finding out from you what your early career was in chemistry or whatever it was before you went to Melvin and how you came to join Calvin's group?

RG: Sure, I had been an undergraduate in Chemistry and Biological Sciences at the University of Kansas and I didn't really hear of Melvin until I took Organic Chemistry and in Fieser's organic textbook he footnoted a number of famous chemists and, of course, Melvin's name appeared there and I was intrigued by some of the things I read there and read a bit more about him. By the time I graduated from Kansas and gone on to an industrial job at Monsanto, I knew I wanted to go back to graduate school and I became very certain that Melvin was who I wanted to work with. So after a little over a year at Monsanto in St. Louis, I got into the Chemistry Department at Berkeley and showed up in California.

VM: As a graduate student.

RG: As a graduate student, announcing that I wanted to work in Calvin's lab. It was then pointed out to me by, it may have been Ken Pitzer, I think it might have been Pitzer who chatted with me and the new students at that point, that the way we do it here is you take a leisurely introduction and you take a good deal of time to decide who you are going to work with and we really don't expect you to jump into a lab.; you take at least three or four weeks before you get started on serious work here. It is very different from the way we do it now, incidentally. You are really expected to be at the bench and doing some research within a month of the time your feet hit the ground in Berkeley. And so I talked to people and it didn't change my mind. I knew I wanted to talk with Calvin and after I met him I really knew I wanted to work in his laboratory. I was assigned to a space in the Old Radiation Laboratory.

VM: Just a second. How did you meet Calvin actually for the first time? Can you remember the occasion?

RG: Yes, I do. He was one of the faculty members I was to talk to in order to decide in whose laboratory I was going to work. He was in his old office in the Chemistry

Building. It was a very large room and I can now remember he had these floor-to-ceiling bookcases, and it was about a storey and a half inside the office itself. He was an incredibly energetic and interested man. I later learned how interesting a man he was. He was an incredibly interested man and whatever you wanted to talk about he would dive into it with real enthusiasm. I remember he was interrupted three or four times during the course of our talk, somebody wanting this, asking this question. At one moment someone came in wanting a particular compound and I remember Calvin coming around the desk, scampering up on a ladder, reaching back amongst some bottles at the top, digging something out and handing it over to this person and then chuckling, "How are they ever going to find all this stuff when I kick?"

VM: This would have been around 1958?

RG: This was definitely 1958, September 1958, yes.

VM: Did he ask you what you wanted to work on?

RG: Oh, he presented a number of problems. And, of course, I made the mistake of going to him thinking I knew what I wanted to do. It was a terrible problem but he let me get started on it, anyway, for about 3 months.

VM: What was the problem?

RG: I wanted to look at DNA and ageing. I wanted to see whether or not there was any correlation between the persistence of DNA and ageing. So I wanted to follow a piece of DNA from a young state into a time when the cell, or the organism, had aged to some extent. I remember Calvin saying, "You know I have been waiting to get into this business and I just can't wait to and Messelson and Stahl have done this work. Let's do it!"

VM: Why did you think that he was the relevant person for that? He had had no DNA experience, really, by then.

RG: I was so unsophisticated I didn't know about that sort of thing, thank God.

VM: Anybody could do anything...

RG: Anybody could do anything. And that was an unusual thing about the lab., as you know, Vivian. In fact, anybody *could* do anything. If you could do it, and wanted to do it and you could get organised to do it, you did it. It was wonderful that way. But you could go very, very far astray.

VM: So you actually started working on it?

RG: I actually started working on it and worked on it for about two or three months and I remember there was a young fellow named Rod Park who had come up from Cal Tech and his responsibility was to do science and to run the algae lab. I can remember talking about this with Rod and Rod sort of clucking that I had experience in this and it would work and so forth and me stubbornly going on for a couple of months and then coming to my senses.

VM: And then what happened?

- RG: Well, then I took up a problem that a fellow named Moses had worked on there for a bit. I was terrified by the quantities of tritiated water.....
- VM: Did you pick that up?
- RG: Yes, I did. It was a matter of working with a couple of curies of tritiated water and I did one or two of those experiments and decided that I wouldn't live to complete them. I decided after a number of conversations with Gabriel Gingras, who taught me an awful lot of plant physiology and introduced me to hydrogen adaptation in photosynthesis, that I wanted to look at the path of hydrogen by using hydrogen-adapted algae and got working on *Scenedesmus*. Fortunately Gabriel was willing to join me on that and we had a great deal of fun with that. It went very well.
- VM: Are you still in touch with him?
- RG: Yeah, we saw him and his wife, I guess a year or so ago.
- VM: Where is he?
- RG: He's at Montreal University in Montreal, and I invited Gabriel down and he gave a nice seminar — I guess about two years ago, actually.
- VM: He's in biochemistry, is he?
- RG: He is in biochemistry and he is a full professor there and so forth and has a wonderful little observatory on top of a lovely house in Montreal.
- VM: So your first working space was actually in ORL?
- RG: First in ORL and then I was moved to LSB where I stayed for the time I was there.
- VM: In ORL, itself, were you in the big lab.?
- RG: I got a desk and sort of did some work in the main room and then shortly after that we moved to LSB, the Life Sciences Building. There is something I really do want to say, Vivian...
- VM: Please do.
- RG: ...because there was an unusual aspect to my career in Calvin's lab. as there would have been for anyone of my racial background at that time. This was 1958; it was five years before the Civil Right Act, six year before the Civil Right Act was passed in the United States and it was a time when there was widespread segregation. I am struck by fact I went to Berkeley and I never felt, at Berkeley, any kind of racial discrimination or racism. I interacted with faculty there, a favourite of mine was Professor Cason who had a southern accent so thick you could cut it with a knife, and it was only after I had been in Calvin's lab. for a couple of years that I realised that a book I had looked at (I think it was *Electronic Interpretation of Organic Reactions*, or something like that) was a book written by a guy named Ferguson who is black and who had been a grad. student of Calvin's. Calvin didn't put his arm around me and say "You know I'm a great fan of the black people and I've had a black student and you'll fit right in; we know how to treat black students here." He just treated me like anybody else with one very important exception, which I think is true of a number of graduate students, and that is: he and Gen seemed to watch the students and find out

what might be needed. I was desperately poor at the time and people always made sure I had enough work so that I could support myself. They arranged a kind of teaching associateship there where I got paid 12 months a year and, of course, I worked as a research associate in the laboratory so that was taken care of. Gen would invite my wife and I up to the house and we would have lovely meals there. She made a point of remembering every graduate student's name. No matter when you called Melvin he would somehow make space for you if you were a graduate student; you'd get in to see him. Post-docs. often had great difficulty seeing him but when a graduate student called they got in. Of course he had, for Berkeley, a small number of graduate students — there were only eight or nine of us at any given time. And I remember he also insisted on teaching an undergraduate course, something called Chemistry 8, and I TA'd on that course for a number of years, for two years for him...no, I TA'd in that course for all three years, actually. I was there for almost three years. He did essentially all of the lecturing in that class in spite of his travelling.

VM: Was he a good lecturer?

RG: Very good lecturer. And we graded the exams but when the time came to assign grades we would come in, all the TAs, and we would tell what grades the students had made and before assigning the grade, particularly if it was anywhere near a line, Melvin had go back and forth and we would chat about it and that sort of thing. I never saw a student lowered but sometimes one would be brought up to some extent. And then after that, something that didn't strike me as at all unusual at the time — I was grateful for it but it didn't strike me as unusual — I lived in what was called the "incubator," which was married student housing about 3 miles from campus. It was called the "incubator" because so many graduate students had these large families out there.

VM: Whereabouts was it?

RG: Out in Albany, California, a town just over from Berkeley. I always bicycled to the University but Melvin always insisted on giving me a ride home. Again, at that time I thought it was nice but it didn't strike me as anything extraordinary. Having students myself and being in that situation I know how rarely I do that sort of thing now. So you had this person who had a reputation around campus as being a real bear. Melvin was not loved outside his own group, as you know. He was a terror as far as people around campus were concerned.

VM: Well, they were afraid of his perception.

RG: They were afraid and I think sometimes for good reason. He had given them reason to fear him. He could be very difficult and very outrageous in terms of the way he would treat people. But in the group, itself, and it was a huge group — it must have been about 60 people — I never saw that, never.

VM: I don't know how big it was at the time you first joined it but certainly by later years it got even bigger, up to about 90.

RG: It was enormous. Now, the Friday morning meetings, I've never quite had anything quite like that. The closest thing I had at the Whitehead Institute (that's where I go for sabbaticals, in Bob Weinberg's lab.) and it comes close but doesn't quite do it. One doesn't have quite the kind of intellectual breadth that one had in the Radiation Laboratory. But as you recall the way those meetings were done, two people would present, you would get up and you would present your research and you would get

stopped every other sentence, sometimes by Melvin and sometimes by another person in the group. And you had to stop right there, dead on a dime, justify your reasoning, demonstrate that you knew what you were talking about, that you had read up on things and it was just a wonderful session and you learned an enormous amount. And as you recall, those were held whether or not — they started at eight o'clock in the morning on Friday and Melvin would look around the table and he would say out loud, "Where's so and so? where's so and so?" and even though attendance wasn't taken if anybody told you Calvin was asking for you, you got very worried and you made sure you didn't miss again.

VM: But by the time that you had joined people were getting notice of their obligation to give a seminar. It wasn't still at that point where he just sat down on Friday morning and looked around the table and pointed at someone and said "you".

RG: No, it wasn't done that way, thank God. You did have some time to prepare.

VM: You had about a day's notice, I think.

RG: Yes. They would tell you during the week you were going to talk this Friday and you would get ready and get up there.

VM: So you found it personally an invigorating experience and helpful — helpful to your own work?

RG: Extraordinarily. Helpful to my own work and I learned an awful lot. Of course, I was terrified when I had to talk. That's the way things were done at Berkeley at that time.

VM: Well, in Calvin's lab.

RG: Of course. After you had survived two or three of these things, well you got kind of cocky and accustomed to it and you sort of looked forward to showing off a little bit.

VM: Who were the graduate students in Calvin's lab. with you at the time?

RG: Well, there was Jan Anderson...

VM: Ah; she was a graduate student. I'd forgotten she was a student. Was Biggins there when you were there?

RG: John Biggins came while I was there; we were contemporaries. Of course there was Gabriel Gingras and Gabriel and I did a lot of work together. And there were others whose names I'll remember if you prompt me but I'm getting to be an old codger now and I don't remember everything.

VM: Well, I don't really remember them all. I have lists from Marilyn as to who everybody was.

RG: I remember very well when you came.

VM: When I came back.

RG: When you came back, and I thought that very well typified that kind of interaction that we had. You had come, and I think you had just gotten back from a trip to Israel or something of this sort, and there was a movie on Israel on and I struck up a

conversation with you and you said you were going up to see this movie and I said, "Can I come along?" We came along, had a fabulous conversation, we got to know each other and interacted a great deal over time. It was the sort of place where you just interacted with anybody. It was very easy to do and you struck up a lot of friendships and a number of collaborations that way.

VM: How did you find this collaborative matter interacted with your obligation to write a thesis?

RG: There was no real problem. I guess I worked on three different problems during the course of my time as a student there and, you know, a couple of times a year I really had to give an account of what I was doing in group meetings and in conferences with Calvin, and as long as your problem was moving along all right nobody really cared what you did so you could have a great deal of fun.

VM: So you kept a sufficiently clear idea of where your thesis was going so that you could...

RG: I was lucky. My thesis led me in the sense of, once I was able to get uptake of tritium gas and demonstrate I could get the uptake and get CO₂ reduction, then, you know, one did the obvious things so the thesis essentially led me right along.

VM: You said a bit earlier on that you decided you wanted to stay alive and therefore didn't want to work with curies of tritium. So I don't remember the details at this point of what you actually did. What did you do?

RG: I started off looking at the path of hydrogen in photosynthesis and thought maybe if I could introduce it as tritiated hydrogen gas it might be a little easier to follow than using it as tritiated water — it might go fewer places. As it turned out, nothing was known about the path of carbon in hydrogen-adapted organisms and so what Gabriel and I ended up doing was tracing the path of carbon in hydrogen-adapted *Scenedesmus* under photosynthetic conditions. And the path turned out to be very much what it is in *Scenedesmus* under normal photosynthetic conditions. And so we essentially characterised and worked out that pathway. In terms of rationalising it, I borrowed Arnon's scheme in explaining what was going on. And I remember Calvin looking at that and just dismissing it out of hand because it had come from the wrong place. It turned out to be that was very much the way things were going on.

VM: Did you know Arnon — did you ever meet him?

RG: Oh yeah, I interacted with Arnon a good deal and with Arnon's lab. I liked Dan Arnon a lot but you almost couldn't mention Dan's name in Calvin's presence because he hated him so much. He loved toying with Arnon and making him look foolish. At one time we had joint group meetings for about — oh, we had about three joint group meetings with Arnon's lab. — but it just didn't work out because Calvin just couldn't stand the man intellectually. But that didn't stop me interacting with him and his graduate students and I got a lot out of it. It was that kind of place. The main thing was, were you getting the job done? Were you doing some thinking? How you got the job done nobody really cared, nobody cared.

VM: So you spent a total of what, about five years there?

RG: I was there a little less than three years.

VM: Oh, that was quick for a graduate student. Most of them...

RG: I worked day and night. When my first child was born I took my wife to the hospital, saw the child delivered and was able to get back and finish my experiment.

VM: Being married and then becoming a father, did that — and working so hard — how did you fit in with the social scene in the lab.? How did you perceive the social activities at that time? To what extent were you able to participate in them?

RG: Well, I didn't participate in any social activities to any extent, not because I had any perception I wouldn't have been welcome. It just wasn't something I was doing at that time. I was basically trying to finish a degree.

VM: And you were working day and night?

RG: I was working day and night and Cyril Ponnampuruma, whom you mentioned at lunch, also had a very similar experience to mine in the sense that Cyril came and finished in 2 1/2 years and Cyril worked day and night. Cyril tells the story about how when Calvin took him into the lab., he said, "I'm going to take you to Donner Lab. and I'm going to introduce you to Dick Lemmon," and he introduced him to Dick Lemmon. And then he said, "Now I'm going to take you to the laboratory," and he took him to the laboratory and he said, "You see this laboratory?" And Cyril said, "Yes." And Calvin said, "This is where you spend all of your time. You leave to eat; you leave to go home and sleep; but you spend all of your time here; this is where you work." And Cyril felt a little intimidated by that. But on his own it came to be the place Cyril preferred to be and he did work day and night and weekends and got a hell of a lot done.

VM: That was in Donner, presumably?

RG: That was in Donner.

VM: But you found other people in ORL and later in the Life Sciences Building where you were, who were also working...

RG: Oh, Gabriel and I worked a great deal. We worked all the time. We were not there alone. Jan Anderson worked a great deal. Calvin didn't tell me and he didn't tell Gabriel and hadn't told Jan, "You must work all the time". But we just worked all the time.

VM: Was this very different from the post-docs.? Did they not work all the time?

RG: The post-docs. worked hard. I don't think they worked quite as much as the graduate students did. In many cases the post-docs. had wives or families and felt they the need to spend some time with wives and families. I was an incredibly sexist person at that time and very insensitive, and I didn't care about what my wife thought or was doing. I only thought about my own career and my own concerns, and so I gave no thought to family responsibilities.

VM: So you weren't really in a position to go on mountain trips...

RG: No, I didn't do any of that, an occasional concert, that sort of thing. But I don't want to present myself as someone who felt deprived or unhappy. I had a job to do.

VM: You had a job to do and you wanted to get it done as soon as you could.

RG: Yeah, and it was great fun. It was terribly exciting. As you recall it was an enormously well-equipped laboratory. I remember they bought this mass spectrometer. It was not a laboratory where they would buy these fancy pieces of equipment and nobody could touch it except a specially trained technician or somebody. They bought the spectrometer and we, as graduate students, and other people were invited to come in and do experiments on it. And it was extremely valuable to me. I got in and ran it myself.

VM: You mentioned your collaboration with Gabriel and you saw Melvin, obviously, on Friday mornings. He was your thesis advisor?

RG: He was my thesis advisor, yes.

VM: Did you get to have personal conversations at any length?

RG: I had personal conversations with him. I could count the times during a year I had any kind of in-depth conversations — maybe five, six times a year.

VM: Did you have to find him? Did he come and look for you?

RG: Occasionally, he would stop by when he came in the lab. and ask everybody how you're doing, so forth and so on, what's going on and a brief conversation there. But in terms of actually sitting down and chatting that would happen a few times, less than half a dozen times a year.

VM: You had to request it?

RG: I would request it. And whenever I requested it, well with one exception...Once or twice a year you would go up to his house and you would have a very nice conversation there, usually not about science, it would be about anything. It would be an interesting table and there would be interesting people there at the table and you would have a great conversation and so forth and you would talk to him about almost anything. Quite often it would be sort of an argument about this, that or the other because he would like to bait you on these things and see what you would say. And he made you feel free to say pretty much anything you wanted to. There were no penalties at these things. I never requested a conversation without getting it but let me tell the whole story, Vivian. I didn't want too many conversations with a thesis advisor because they might have tried to tell me what to do! I wanted a conversation with my thesis advisor when I had results to present and a story to tell. I didn't want somebody looking over my shoulder telling me what to do.

VM: But it was going well, presumably, and so you felt you knew where you were going and how to get there.

RG: Yes. And there were other things I wanted to do aside from my thesis and I wanted the freedom.

VM: When it came to writing the thesis itself, and getting it drafted, did he look at the drafts that you'd written or was that all your responsibility? Did anybody look at the drafts?

RG: He looked at the first draft and thought it was OK. I was rushing because I was trying to get to a job at duPont and I rushed him enormously about that and he got rather angry with me and I remember him snapping, "You can't just expect me to do your bidding at the drop of a hat!" And other people on campus — Bill Dauben looked at it and Tobias looked at it. And Bill Dauben didn't like it and sent me back to the drawing boards on it and bless him for doing so because, as a consequence of Bill Dauben's prompting, I put a much better theoretical section in it. I had no kinetics in it the first time he read it and that sort of thing.

VM: Had you left by then?

RG: No, I was still there. I wrote it much too quickly — it was just one mad dash. I wrote it in a month.

VM: About this period. You say you had a job waiting for you at duPont?

RG: Yes.

VM: How did you get a job?

RG: Melvin had hoped that I would take a post-doc right there in town, in Albany, in x-ray crystallography. I had gotten interested in x-ray diffraction work after Kendrew had published his paper and I thought that was what I was going to do and he had arranged for me to meet this guy over in Albany that ran an x-ray diffraction laboratory.

VM: Was that in the USDA?

RG: That was in the USDA, yes. And I was pretty well all set to take a post-doc. there and then I got to worrying about the family finances. The second child had come by then and an interviewer from duPont came through and I decided I was going to go to work in industry because I wanted to make some money. I had no sense again that it might be hard to get a job at duPont because I was black and the interviewer was there. I needed a job to make me some money so I went to the interview and sure enough they hired me. When I told Melvin (he was a consultant at duPont), he said the part of duPont I was going to, CRD, was a very good place, good people there, but he was disappointed I wasn't going to do the post-doctoral work but it was a good place and when was I going? Well, I thought, maybe three months from now. "WHAT? You haven't even written a thesis!" Well, I thought I could get it all done. "Well, that's an awfully short schedule." Well, anyway.

VM: Was he right?

RG: I got it done but I irritated him because, you know, when I got it done I stuck it under his nose and believe it or not a person who runs a group of 60 and has consultancies all over the world has other things aside from worry about one graduate student's schedule. But anyway it got done.

VM: You also mentioned earlier on, as we were having lunch, that you had been friendly with Karl Lonberg at the time...

RG: Oh, Karl Lonberg and I...

VM: ...and, of course, he also went to duPont.

RG: I told him about duPont and Karl came and looked and liked what he saw...

VM: I see, it was because of you that he went there?

RG: I like to think that I was somewhat influential in persuading him to come, yes.

VM: What was the nature of your interaction with him in the lab.? Just as friends?

RG: Just as good friends. Karl was a very liberal guy — very far to the left...

VM: Very much so.

RG: ...so again, looking at it in hindsight, I can see why Karl...Karl actually made friends with me. Karl is something of a legend around the place but a liberal like Karl sees somebody like me in the lab. and he seemed to like me but a liberal like Karl would see somebody like me in the lab. and immediately he would come to make sure a black person was being treated right and all that. There wasn't any problem. And, you know, it turned out I liked him enormously as a human being — very, very high scientific standards, very smart, granite sense of integrity, a bit of a stick, really, on the integrity part, but he was so much fun you could let that go by. Karl was always a lot of fun; at duPont he was a lot of fun.

VM: So you were in touch with him in duPont.

RG: Oh yeah, we were colleagues, yeah.

VM: You were in the Central Research Department?

RG: We were both in the same group in the Central Research Department.

VM: What have you done since then, since duPont?

RG: I left duPont after five years and went to Yale as a visitor, came back to duPont and then went back to Yale.

VM: They gave you, as it were, a sabbatical or leave without pay?

RG: They let me go for a year and then welcomed me back and then I left again. And spent a very pleasant and exciting time at Yale in the Biology Department there...

VM: As a regular faculty member?

RG: ...as a regular faculty member..

SM: Which year was that?

RG: That was 1966-1970 and then I went off on sabbatical to Harvard and then came back and they made me Master of College at Yale. It was apparent to me that I wasn't going to get tenure at Yale — I really didn't deserve it — but I could have stayed on as Master of a College there.

VM: Which means what?

RG: It is very much like Master of College at Oxford or Cambridge.

VM: Oh, I see. A residential college?

RG: A residential college and we set some of our own courses we offered and that sort of thing. As you would perceive, at that time the United States it was very much like it is now or even more so. Being a black person who was in the sciences, there weren't many of us and socially-conscious institutions were very interested in having some of us around. So Yale would have kept me around as a Master of a College or something like that. But there was an opportunity to go to the University of Maryland and remain in science, which I much preferred to do. Cyril was down there and Cyril really greased the rails and arranged for me to come to Maryland. And so I was at the University of Maryland for ten years, although I spent three years of that time out in California at NASA at Stanford.

I left Maryland for a Chair at Amherst College, which I occupied for, I guess, about four or five years, and then they asked me to come to the University of Massachusetts.

VM: That was about when that you came?

RG: I came to Amherst in '82 and I came to U. Mass in '85, I guess it was, '86, and was at U. Mass from '86-'89 when I went back to Amherst as the Simpson Lecturer and Professor and have been there ever since. I still hold a joint appointment here at U. Mass and I get my graduate students from U. Mass and my undergraduates at Amherst. Amherst is a wonderful little college, as you know. And U. Mass has some very good departments and some very strong departments. It is a very nice set-up that I have between the two places here now. I think I have finally found a home here.

VM: What manner of contact have you retained with the group in Berkeley — the people or the place itself?

RG: The last time I saw Calvin was 1983 when I invited him to come here and give a seminar. He came and gave a couple of wonderful seminars, one being a public lecture on the use of the *Euphorbiaceae* as the sources of oil. That was a general public lecture and he really made it crackle and really interested people, the kind of thing (*where*) you wanted to put down what you were doing and start working on that. And then he gave a straight-forward talk to a combined chemistry and biology group on photosynthesis, which was a wonderful and elegant lecture.

As you know, Melvin was able to make things very clear whenever he wanted to and the excitement just came right through on what he was doing and what he was thinking about. There was a kind of — naive is a term I hesitate to use because of the extraordinary scientific capacity and sophistication of the man — but there was a child-like quality of the way Melvin went after things and talked about them and dealt with them. It always told he was interested because he was just open and unbridled, he wasn't trying to protect himself at all when he talked about something he was really fascinated in. And when you combine that with a kind of extraordinary — he was very, very smart and very, very quick and knew an enormous amount — he could quickly synthesise all kinds of things. It was just a wonderful performance.

I remember once when we had him back for a seminar at the University of Maryland. one of our younger physical chemists made the mistake of thinking he was dealing with what the physical chemists dismissively called "biochemists"! Melvin was

giving a lecture on photosynthesis and, of course, when you are talking about processes in photosynthesis you don't write balanced equations, for God's sake, you put down reaction flows. And Melvin was showing flows of reactions in carbon and so forth and I remember Miller stopped him and said, "Uh, uh, pardon me, Dr. Calvin, but you know I'm not a biochemist and I usually deal in balanced equations" — and Melvin was putting something speculative up about interactions between manganese and oxygen-like driven(?), a rather complex piece of chemistry — "and I can't really understand that reaction unless it's balanced". And Melvin flashed, you could just see the anger flare. It happened for just a microsecond, just like that, and then he calmed down again and he said, "You wait for your electrons for just a moment or so," and then he went on with the lecture, all the time, I realised, working this out in his head as he went through the lecture. At the end of the lecture he wrote off this elaborate balanced equation and said, "There are your electrons, are you satisfied now?" The audience was just stunned, just stunned. And, of course, Melvin could do that sort of thing. I remember when he was here for the lecture, we didn't want to burden him with a lot of conferences with people so when he wasn't actually talking Barbara and I just took he and Gen around the area and had a couple of very nice days wandering in and out, here and there, going to tops of hills and taking pictures, that sort of thing. I remember I thought the afternoon had been a little strenuous and I wondered if they wanted to come back to our place for a cup of tea. We had a log cabin at that time and we went in for tea and they commented on the place and how nice it was and so forth and as we were having the tea my wife asked him a question that had to do...my wife is not a polymer scientist she is a very good molecular biologist but she's by no means a polymer scientist. She asked him a question having to do with some aspect of polymer science because she was curious about it. And Melvin sat there with his cup of tea and gave the history of the field, gave its current development, pointed out where it might be going, all in the space of about five minutes. She wasn't a chemist but she understood everything.

So that is the long way of saying he gave a couple of extraordinary lectures here which went over very, very well and by that time I was old enough to really sense what kind of person I'd had the privilege of interacting with. As a graduate student I knew he was smart because, you know, I TA'd for him and I watched him at group meetings and so forth and I knew how he could ask me questions that could give me difficulty. But by the time I was in my second year and so forth I knew the system I was working with better than he did and sometimes I would get impatient with him...well I told you that last time. I wouldn't say that, of course, but I would think, I told you that last time, can't you remember anything? And we'd go on. So, you know, as young people often don't really know what they are interacting with until they look back.

VM: What was TAing for him really like? What did you have to do?

RG: Well, again, it was a wonderful situation because in this course he taught called Chem 8 he didn't have wet laboratories he had discussion groups, instead. His feeling was that students can't really learn chemistry unless they can talk about it and there was no way he could talk about it with 300 people. So he broke it up into small sections and had TAs meet with about 25 or 30 kids once a week when you would really go through in some detail.

VM: The same group?

RG: Each TA would have a group of 30 kids, 25-30 kids, and you would meet with these kids and would go through a list of topics that had been raised in class. So Melvin

would have TA meetings and he'd say I want you to talk about this or I want you to check them out on that and I want you to do this and you'd get together with your group of students and you would take them over. Melvin was very clever. He would tell us, "you know how they do on exams is really importantly influenced by the quality of TA they have and the good TAs tend to get their kids to do well and so forth and the ones who don't take a good attitude, their kids don't do so well". What he was doing, of course — looking back over the years I now know exactly what he was doing — he wanted us all to put as much into it as we possibly could. So you got this group of kids, once a week, all in a room all by yourself and you were teaching them. You would kid yourself to think you had a lot to do with how much chemistry they were learning. Well, you did have something to do with it and so you got to really teach. I came to love teaching there and I learned something about how to teach during those years I worked as a TA in his class. So that when I really found myself in front of a class I knew a little something about how to teach it.

VM: The other thing I would like to bring up is where you worked. You worked in ORL and in Life Sciences....

RG: I worked for maybe a month or so in ORL...

VM: But long enough to see what it was like.

RG: To see it was an incredibly decrepit building. I thought it was going to fall in at any moment. The fact that great science had been done there amazed me.

VM: Did you stay with the group long enough to move into the round building later on?

RG: No, no...

VM: Have you been back and seen it?

RG: I have been back and seen it and I have been back and done a little work in the round building. I was on sabbatical; I stopped back and did some work there.

VM: Some people have spoken a lot about ORL and what they think it meant to the way the group developed. How did it strike you? I agree you may have been only in it for a few weeks.

RG: I think I probably wasn't there long enough to be indoctrinated into the mystique of ORL. The thing that people talked about a lot in LSB was the fact we had a big white table that everybody would gather around for coffee or for tea at ten o'clock in the morning and Melvin would often show up at those and, of course, whenever he came the spotlight immediately moved to him and we kind of stood around and listened and so forth and sometimes learned about very new things that he had seen in his travels and that sort of thing.

As I look back from this distance now I marvel at how he was able to juggle the travel schedule so that he was really there for the Friday morning meetings. You essentially saw Melvin on Friday morning, every Friday morning. Now how do you do that with the kind of travel schedule he had? He would always arrange to be around LSB at least once a week at a time that wasn't Friday morning and he had a heart attack while, during the time I was there and I remember they got this golf cart for him and he would go up and down the hill between his office in Chemistry and LSB on this

golf cart and so forth. The car would whiz in and so forth. You could drive it right into the lab. there in LSB and he would make the rounds of people.

VM: So even in this place with a long corridor and separate rooms you felt there was a considerable sense of community and the white table was there...

RG: Oh, there was a considerable sense of community. And we just had an awful lot which made us very much our own institute. I guess if there was a down side to some extent to being in Calvin's group, it was that you didn't interact much with the rest of Chemistry. You were very much part of an institute and very much part of a self-contained unit that pretty much had everything it needed. We had our own glassblower — so you didn't have to wait in line to get our glassblowing done — you occasionally went to the Chemistry supply house but basically you ordered things that came into the laboratory there. The budget was unlimited and so you just went to it.

VM: But you did describe how you made your own contacts with Arnon as an outside group.

RG: Right. But he was there in LSB and we needed him because he was a thinker in photosynthesis.

VM: Of course, he was physically very close.

RG: He was right up on the third floor, yeah.

VM: So when you went back and saw the round building and to a degree worked in it, how did that strike you?

RG: It was a magnificent space. One of the best work spaces I have ever seen. Just magnificent.

VM: That says it all, doesn't it? OK. What sort of contact have you had with other people in the group? Because you mentioned you last saw Calvin in the early '80s but have you kept up with other people? I mean Clint, here, is a neighbour.

RG: Here's Clint.

VM: It is a very dispersed group.

RG: Well, I've seen Rod Park a couple of times and had dinner at his house and drank some of this incredible wine that he makes, which is wonderful, wonderful stuff, it really is.

VM: He wants to go back and spend a fair amount of time on his winery, he told us a couple of weeks ago.

RG: I hope so. But as for keeping in close touch with people, no it really hasn't happened. Cyril, of course, we were very close, and Karl Lonberg in his time at duPont we were very close. But aside from that...Gabriel and I lost touch with each other for maybe fifteen years.

VM: But it clearly has been an important stimulatory and educational factor in your life.

RG: Critical events in your life, obviously, as it was for so many people. I just wanted to say a word or two more and just chat a little about Gen Calvin. It wasn't that you saw a great deal of Gen but it was almost routine, when you were in Calvin's office or something, that either he would call her or she would call him. They were obviously close and when you heard the conversation that went on it just told you they were a real working partnership. There was just no question that she made a thing of getting to know something about the personal concern of people who were in Calvin's laboratory and trying to be helpful and trying to be supportive. It meant a great deal when she called you personally, when she actually asked and actually listened to what you were saying. When you saw her for dinner and she was warm and friendly; when you saw her in later years when she was travelling with Melvin — they travelled a great deal together. When one sneezed the other covered their mouth, it was that kind of relationship you would see between the two of them.

It was a great shock when she preceded him into whatever happens next. I don't think he ever thought it would go that way and I don't think she really thought it would go that way. But I thought she was a very important influence. As I think about the lab. itself, there were other critical things that made the lab. such a supportive environment, I thought. Marilyn, of course, knew everything and knew everybody and was a real white witch, and they are the good witches, as we all know — angels, in a sense. Marilyn was just...she just had to be digitally put together because she never forgot anything and she always got everything done.

VM: She still hasn't forgotten anything.

RG: Just an amazing person and also was a person who made you feel very welcome and a sort of a friend to all the graduate students, I think. And then the support staff we had around the laboratory, people like the glass blower, people who did carpentry or mechanical work that needed to be done. You could imagine a piece of equipment there and within a week you had it working. So you could create an apparatus to do the experiment you needed to do. It was just a wonderful place to work.

VM: Have you come across other places like that?

RG: Yes. The one place I've come across places like that is the Whitehead Institute that's attached to MIT: again, an extraordinary working environment, very, very good people, all the resources you need essentially in one building to get done what you need to have done and a crackling scientific atmosphere.

VM: Was it the creation of one man in the same way the Berkeley group was?

RG: No. It was the gift of Jack Whitehead, of course, who put the money there to let it happen and the fact that he continued to water it during his life and in his bequest and also spiritually to be around to encourage people. It's very much the creature of the six or seven founding members who felt that they were going to get all these egos in a very small space and that what they would have to do is to maintain an atmosphere where people felt safe to be open about their research. So you have people talk about research in progress in an incredibly competitive field like molecular biology and still pretty much be able to say out loud what they are doing to people and have it stay right there. The other thing they have pretty much succeeded in doing is keeping all those egos in check so they don't tear each other apart. The Whitehead turns out to be a rather pleasant place to be socially — it's very cordial, people don't fuss and fight a lot.

VM: It was, of course, founded considerably after Calvin's group.

RG: Oh my, 1982-'83, something like that.

VM: Yes. Perhaps as an example of that sort of thing Calvin's is very early, maybe one of the early ones.

RG: It is very early. And also, I think, very different. Because, as you say, Calvin's lab. was very much the creation of one person. It would not have existed without him.

VM: OK. Thank you very much, indeed, and I hope it won't be quite so long before we see you.

RG: I hope not, Vivian.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Richard Allen Goldsby

Date of birth 12-19-34 Birthplace Kansas City, MO (USA)

Father's full name Howellyn Emmett Goldsby

Occupation Insurance Sales Birthplace Truckee, CA (USA)

Mother's full name Laverne Louise (Walsh) Goldsby

Occupation School Teacher Birthplace Fordyce Arkansas

Your spouse Barbara Anne Osborne

Occupation Professor Birthplace Laurel Springs, NJ (U.S.A.)

Your children Jeanne Louise Goldsby John Stuart Allen Goldsby
Robert Allen Goldsby, Marjorie Maureen (Goldsby) Prete

Where did you grow up? Kansas City, Missouri

Present community Amherst, Massachusetts

Education B.A. (Chemistry) University of Kansas

Ph.D. (Chemistry) Univ. of California Berkeley

Occupation(s) Professor (Teaching and Research)

Areas of expertise Immunology

Other interests or activities _____

Organizations in which you are active _____

Chapter 32

R. CLINTON FULLER

Amherst, Massachusetts

July 24th, 1996

VM = Vivian Moses; CF = Clint Fuller; SM = Sheila Moses

VM: We are talking to Clint Fuller on the 24th of July 1996 in Amherst at the University of Massachusetts.

Clint, can we start the discussion by your telling us what life was like for you before the Calvin adventure and how you came to participate in it?

CF: Well, what life was like very early is really not relevant to my scientific career because in my other always wanted me to be a doctor, a real doctor, and I struggled all through my youth and adolescence with that held over my head. Luckily when World War II arrived I was drafted directly out of high school and at the time I went into the fleet to become a medical technician in the Navy. But at the time there was a programme where, if you passed a certain examination, the Navy sent you back to college and that was part of our so-called "V-12" (V for Victory, thanks to the British) programme and I was assigned to Brown University in Providence, Rhode Island, and had a great teacher there on the way to Medical School, I thought. The great teacher talked me out of it. It was George Kidder, who ended up here at Amherst that was a good friend of Melvin's. He had almost an earlier influence on me than Melvin did. So when the war was over we were dismissed and I didn't graduate by that time and followed my professor to Amherst, Mass., where I got both a Bachelor's and a Master's degree from Amherst College.

VM: In Chemistry?

CF: In Biology. I was really a cell biochemist.

SM: What year was that?

CF: I got my degree in 1948. And, incidentally, I was here in '46 right after the war. It was a town where — I met my wife at college, at Brown University — we were married here in Amherst. It essentially took me 23 years to get across town where I am now Professor Emeritus at the University of Massachusetts. Between that, George Kidder wanted me to go to the very top lab. in the country to get my Ph.D. and he sent me to Stanford where Ed Tatum (of Beadle and Tatum, [*incomprehensible*]) had just gone from Yale and I joined that group at Stanford and spent four years there

getting a degree in what was then called “One Gene, One Enzyme.” Molecular genetics had not been formed. I was very fortunate and had a wonderful experience and that is how I got associated with Calvin’s lab. The story is one of serendipity — being the right person at the right place at the right time. It was the spring of 1952, the Korean war was finishing, we had inflation, things were rationed, jobs were terribly hard to get. We had very good relations with our professor Ed Tatum, later a Nobel Prize winner himself, and he was in charge of Sigma Xi, a lecture programme at Stanford, and he invited Melvin Calvin down from Berkeley in 1952 to talk about what was then very exciting, the *Path of Carbon in Photosynthesis*.

Well, before the evening lecture Ed Tatum had his students over to his house to meet Calvin and to have a few drinks and a barbecue. And Calvin, again in a very gracious — I had never met the man but, of course, I had heard of him — in a very gracious way, came around and talked to us individually and introduced himself to me and said, “Well, young man, I understand from Ed Tatum that you are about to finish your degree. What do you plan to do with the rest of your life?” (That was a direct quote from Melvin. In retrospect I can remember.) And I said, “Well, Professor Calvin, I really don’t know but I’m looking for a job.” Mind you, this was May and I got my Ph.D. at the end of May and my fellowship ended and things were tough: wife and two of my four children and...Melvin said, “Oh, that’s interesting. You’re a microbiologist are you not?” I would have said “yes” to anything at that point so I agreed. Van Neil was on my Ph.D. committee so I had (*incomprehensible*). He said, “We’re having some trouble with contamination in our algal cultures up in Berkeley. I wonder if you would be interested in coming in to help straighten us out?” Those were his words. I assumed that was sort of a probe for some sort of a position; I didn’t know. And I said, “Well, I would certainly be interested. I’m very excited about what you are doing.” And I said all the right things. And Melvin said (this was on a Friday), “Can you come up and we will talk about it on Monday in Berkeley?” And I said, “I certainly can.” And I went up to Berkeley and we had a great talk in Melvin’s office, with Marilyn Taylor taking notes in the office, which I thought was interesting. A new experience for me. And Melvin said that they were really very much interested in getting the culturing situation straightened out and “if I was a good microbiologist, and if you had worked with Tatum you must be, and we have a number of projects which were completely dependent on having algae”. Now mind you, this was about four years after the *Path of Carbon in Photosynthesis No. 1* came out, published in *Science* where, essentially using C¹¹ work, the early products of CO₂ fixation were all malic acid, oxalic acid, the reverse of the Krebs cycle. That was published. And, of course, as we know, that was completely wrong until it went full circle back to some of Arnon’s work and so forth. So he did need some help because some of his cultures of algae were 50% yeast. That accounts for the Krebs cycle. So, in any event, to make a long story short, he offered me a job on the spot. I will never forget it — \$6000 a year!

VM: You mean he did that without even consulting Andy, who was running the...?

CF: I met absolutely nobody else in the lab. that day. When I came up to see the lab. and look for housing and all that sort of stuff it was all very rapid. I was on the job in three or four weeks. Then I met everybody.

VM: You hadn’t seen the set-up? You had simply talked with Melvin?

CF: He had taken me through the lab. and I met people but it didn’t mean anything — we didn’t spend any time at it. But I did have long talks with Andy and it was quite clear my job was to get going on these cultures and to straighten things out. And the future

was wide open. He didn't, as Dick Goldsby has said, it was pretty much up to me. And that was my Ph.D. experience and that looked good to me.

VM: Can I ask a couple of questions at this point?

CF: Sure.

VM: One of them is that Melvin offered you a job without making it a short-term job?

CF: No. He made it quite clear that I wasn't coming in as a post-doc., I was being appointed as a member of his staff and I would initially get a year's appointment at \$6,000 a year.

VM: It could be renewable or....

CF: Something like that; I don't remember the details. Of course, he didn't have a contract for me to sign or anything but he said, "yes, I would like to hire you, and I'm going to proceed with this" and Marilyn was taking everything down.

VM: Who had been running the algal culture set-up beforehand?

CF: A technician by the name of Louisa Norris. Her husband also worked in the lab. In straightening out the cultures, it wasn't necessarily Louisa's fault that they were partially yeast. It was just that the microbial technique was not too good and I pointed out that we did we would plate out cultures and isolate single colonies, pick single colonies, these are the *Scenedesmus* or the *Chlorella* and grow those in small flasks. We made some changes in the shaking apparatus that grew the algae and things were fine. We had pure cultures. That's when the C¹⁴ work was at its peak.

VM: And Louisa continued to work with you?

CF: Yes. She worked as my assistant, research assistant, essentially. She was so delighted to have help. She was a wonderful person, very nice, and there was never any competition or she would be doing something wrong. Calvin wanted a trained senior person on his staff that knew the biological story.

VM: So, when you started, your obligation was to clean up the cultures and run them effectively and the research was to be whatever you made it?

CF: Pretty much so. Except we had a visitor from South America. His name was A.O.M. Stoppani, from Buenos Aires, and he was one of many international people who came through the lab., and Calvin wanted him to come up with a project while he was there. Well, I had had some photosynthetic prokaryotic experience because I had spent a lot of time with Van Neil because he was on my committee and I knew a little about photosynthetic bacteria. And I said, before Arnon got involved in this, I said, "Melvin, we ought to have some work now you've done higher plant leaves and then *Scenedesmus* and *Chlorella*. Let's take a look at some prokaryotic bacteria. Andre was interested in this, this was his training, in microbiology. And so that was my first project. Stoppani was there for a year, and we published a paper on the *Path of Carbon in CO₂ Fixation in Rhodospseudomonas spheroides*. Calvin was impressed because it did begin to show some of the same compounds but with a different emphasis on amounts. It showed a lot of what Calvin first saw in yeast. In those days it depended on how you grew it but if you grew them photosynthetically, they were showing the early ribulose diphosphate and PGA ions and so forth, but a lot of malic

acid, a lot of oxalic acid and so forth, so that there were a lot of products there that weren't there in the algae.

VM: But you also had to learn all the local technology, chromatograms...

CF: Oh, yes. There was Andy and Al Bassham and they were just great, pitched in. And we did that all in the Old Radiation Lab. (ORL) and many of our colleagues, several — Roger Stanier, Stan Carson was there — a number of people in that era took chromatograms out in the upstairs attic in ORL, breathing benzene and saturated phenol papers and so forth, and there have been several deaths from liver cancer that...

VM: And you think some of those originated from that?

CF: It was an era when, not just in that lab., but a lot of other people. Stanier died of this, Stan Carson, an assistant of Van Neil. A lot of other people have. I've been lucky, I guess. In any event, so the project with bacteria was a start for me because I spent my career with photosynthetic bacteria after that and it was absolutely wonderful. Dr. Stoppani came to visit me and went back as head of the Biochemistry Department at the Medical School at the University of Buenos Aires and went on to be an elected member of the Academy of Sciences and then was President of the Argentinean National Academy of Sciences when he retired just a few years ago. I visited there with him. So that Calvin connection was...people out of Calvin's lab. just became distinguished; they were distinguished when he got them. Calvin was amazing at selecting his people — he selected winners.

VM: At the time you went there, there were the two separate locations, in Donner and in ORL. Who were your colleagues, who were your senior colleagues in ORL?

CF: Andy Benson; Al Bassham wasn't really senior as much but had been there in Calvin's lab. as a grad...

VM: He wasn't a post-doc., he was a staff member.

CF: He was a staff member. Andy Benson. A fellow by the name of Bradley.

VM: Dan Bradley.

CF: Dan Bradley. He was on the staff; stayed there quite a while. Those was the core in ORL. Of course, over in Donner the people at that time were Bert Tolbert, who headed that sort of group, Dick Lemmon. But we never had any real scientific interaction. That is, they never taught me the technology or...

VM: But you knew the people well enough.

CF: Absolutely. We met at the Friday morning meetings and Calvin was always there and one of the people had to speak. It was there that Calvin first scared me, at the very first couple of meetings I went to, in that he would just hammer at the speakers and so forth and only in retrospect do I realise he was doing, as we've heard, I think he was kind of trying to get the very best out of you. He couldn't care less what you thought of him. He wanted to dig out the best and his technique was to beat on you and make you go over and over, and in utter exasperation I used to, ugh, almost dread it. But he had a method and it was terrific. In my early days in that lab. there Calvin did expect you to work all the time. Now I had a wife and two kids, a third one on the way, just

bought a house, I wasn't a post-doc. but was on the staff: didn't make any difference. I perhaps disagree with Goldsby a little bit on that about the senior people. One of the examples would be Sunday morning at eight o'clock the telephone would ring at home and it would be Melvin for me. My wife would usually answer the phone because I was usually asleep and perhaps had had a little too much to drink the night before and partied up a bit. Melvin would simply say, "Clint, I've just been reading over your notebooks and I want you to come down to the lab. right away." And there went Sunday morning. And very probably he would call somebody else who was working with me, maybe Stoppani, and we would be there and go over the books and it was incredible.

VM: So, in your experience, he went over the experimental material in sufficient detail so that he could absolutely talk nuts and bolts about it.

CF: He was on top of everybody's work. It was incredible, the amount of material he could absorb.

VM: So your main base in ORL, then, was that on the upper floor where the algal cultures were?

CF: Yes.

VM: And at the point when you were there, what was the way in which the algal cultures were grown?

CF: They were grown on his shakers that were designed by Calvin and made by his glassblowers, which were water-cooled shakers with Lucite bottoms in the tanks. They were mechanical shakers with...

VM: These were flat-bottomed flasks, rather shallow?

CF: Flat-bottomed flasks with ground glass tops and you could bubble air or nitrogen or CO₂ through and when I got there that was it, that didn't change. It got cleaned up and we kept things like sodium azide in the water simply to lower the contamination and process and treat it microbiologically. So that's where the algae were grown and harvested.

VM: You would be interested to know that one of those flasks survives. It is in a closet in Berkeley. I'm not sure of the fittings but the glass vessel is there. By the time I got there several years after you started, they were also growing them in continuous culture with fluorescent tubes and vertical tubes. That wasn't your development?

CF: No. The steady-state apparatus was just being developed at that time by Al Bassham who really worked on that and developed the whole thing. And, of course, that was the famous story of the fisherman came out of the development of that steady-state growth apparatus with inserted tubes and large 25-gallon bottles.

VM: Is this the time to explore the fisherman?

CF: Well, yes. In 1954, I believe it was, Al Bassham had been working very hard to develop a so-called steady-state apparatus where the algae would be exposed to CO₂ continuously, which was generated in a completely sealed apparatus. And then we changed environmental conditions. If you deprive them of CO₂ and look at the transients, what happened, what would pile up? It was the time for the hunt for the C₂

acceptor. The steady-state apparatus essentially allowed you to make these environmental variables which would show what was happening to carbon when you took out key pieces. And it was very complex and very important and it was really the breakthrough where the path of carbon was once and for all — that apparatus did it. But it was complicated. It was one piece of glass machinery. You had places to inject bicarbonate in tremendous amounts because we would use up to, it seems to me, 5-10 millicuries of C^{14} because it had to be circulating gas at very high activity because they (*the algae*) were only taking in a small amount during the exposure. But anyway, it was very complicated and Melvin wanted a sketch of it drawn up to put in a publication. It was the *Path of Carbon in Photosynthesis*, I believe 22 or 23, whereas the one that was all yeast was number 1.

VM: It was neither 22 or 23 because I wrote both of those. It was probably 21, I think.

CF: 21, OK. That much had happened since 1948. But anyway, Alice Holtham, the secretary, artist, general house mother in ORL — she was sort of Marilyn's duplicate but she was everything from secretary, artist, manager, just a wonderful person — and she drew up this apparatus, very, very carefully.

VM: So somebody sketched it for her to do a proper drawing?

CF: Yes, that's right. And Melvin followed it along and Alice would have it and so forth. And it got to be so complex and everyone said and I said and she would show it to us in the lab. and say, "Does this look OK? Melvin hasn't seen it yet" She had it down pretty well but we said, now look, when this nice big picture gets reduced down to one column in the *Journal of the American Chemical Society* nobody with a magnifying glass is going to be able to read it to see what it is all about. It is sort of useless. But Melvin was adamant, he wanted it published. I suggested (Andy was in on this, too, because he knew Alice pretty well and could get this), let's do a little doctoring for the fun of it. Alice was a little hesitant about this, I think. So the lovely main tank with the bubbles of CO_2 coming into it, right on top of the tank, we had Alice sketch a little stick figure of a fisherman with a fishing pole and a long string with a hook going down and a goldfish, right where all the CO_2 bubbles were. It was wonderful. People in the lab. got a kick out of it. But when Melvin came over to look at it he set it down on the round white table and everyone was around talking. I don't know how many people in the lab. knew about it but Melvin didn't. He very carefully traced the whole thing out, looked all through, went right over the fisherman, never flinched, never saw it, I am convinced now. So it was submitted to JACS and the first thing we knew everything was fine and it was published that way. I think it was a first for the JACS to have somebody fudge with one of their figures, not even a reviewer picked it up. (*Laughter*)

VM: Do you know what Melvin's reaction was when he must have realised or somebody showed him?

CF: He was delighted.

VM: Was he?

CF: Yes, he was absolutely delighted. I think he would have been a little upset if one of the reviewers had said, "Don't fool around with that," or something. Back at his reunion in 1991 (*Editor: actually 1989*), or whenever it was, in Berkeley I showed that slide and it brought down the house again and Melvin was the one most pleased to see it.

- VM: I would have thought that would have generated a tradition of jokes in papers but I don't recall that there were any others.
- CF: I recall another one; it was great. When we landed on the moon in America (*sic!*) there was an article published in *Science* called the "Composition of the Moon" and they gave all the metallic substances in kinds of rocks by sound transmission, as they did in those days. And the very last column in the table was Gruyere in sound transmission. Right there. Which, of course, is Swiss cheese and the moon is made of Swiss cheese and that got by; it's a similar kind of thing.
- VM: Yes. I think the very dry jokes are the ones you want, the ones that slip by, you hope, slip by...
- CF: I was sort of worried if Calvin knew I had participated in this. But that goes to another little incident in the lab. like this when...everybody knew in the lab. that Calvin was down the pipe for the Nobel Prize. That was right at the top of his agenda. He knew he could do it, we knew he deserved it, the brilliant man that he was. He hadn't really accomplished what he wanted to accomplish and that's why to complete the Path of Carbon was so important. That is what he was banking on. He would come in for the results every morning with Marilyn, at ten o'clock, with Marilyn around the table at ten o'clock. I set up a little flag on a ring stand, tall ring stand. It was a pennant with one bell on it and a little label on it said, "This is the Onebell Prize which is much more valuable than the Nobel Prize". We had a little plot in the lab. At first we wanted to put it out on the white table so he would see it in the morning but we decided that was a little direct so we had it sitting off to the side, but there, and every day we'd raise that pennant to half mast or up close to the top or drop it down to the bottom depending on how close it was to the Nobel Prize. But we said we would give him the Onebell Prize anyway as something to shoot for. He finally saw it and sort of smiled but never reacted very much.
- VM: I wonder whether after that he came in and surreptitiously looked to see whether...
- CF: (*Laughter*) I bet he did.
- VM: ...and where he would have thought how you would assess his chances on that particular day. You think it was very much on his mind and he was...
- CF: He never talked about it at all but you can see afterwards that half his perspective book is on the Nobel Prize, the ceremony, his family, and he really was...he never talked about it directly but he was very sensitive. He would talk about other people's Nobel Prizes because he was surrounded by them in Berkeley — they were getting them *en masse* at that point.
- VM: I am sure that the time when you were there in the early '50s was a little early because the cycle had not been resolved. By the time I was there, three or four years later, it had essentially been worked out and I remember there was a measure of tension each year around November time as the results...until finally fruition and joy...
- CF: Yes, 1960, right (*Editor: actually 1961*).
- VM: ...and it all worked out.
- CF: And President Kennedy.

- VM: Well; so you were involved...we talked about your science as far as your involvement with Stoppani and you were working on the *Rhodospirillum* with him. And then what; I mean, how did it develop?
- CF: Then we got into...this is an interesting collaboration with Dan Arnon. And I think we know the story of Dan's relationship to Calvin and vice versa.
- VM: Well, I don't know to what extent you can actually illuminate it. We know there is antagonism, of course, between them. We have not been able to discover how it started.
- CF: I don't know. My guess is that it was straight personalities. They were both, in very different ways, extremely driven people with high focus and they came head on and I really can't answer your question except that they were not on speaking or communicating terms. But they didn't mind if we...he didn't mind if we... And that was the time when Arnon isolated the chloroplast that could do photosynthesis for the first time. My next project was one that has been described by Andy Benson and involving what went on as a result of the beginning of the steady-state experiments and so forth. I had done some training in enzymology as part of my Ph.D. thesis and knew my way around enzymes a little bit and that was the time when Andy had finally convinced Calvin — and believe me it was blackboard sessions, Calvin's driving — that $C_5 + C_1 = C_6$ divided by 2 = C_3 .
- VM: Can I pursue this a bit because...
- CF: That was finally accepted.
- VM: Right. You were there at the time there were arguments about pluses and minuses, 3s and 4s...
- CF: Transketolase, transaldolase all the movings and C_2 s.
- VM: Can you remember how it started? Can you remember...
- CF: Yes. Andy Benson. He should have shared that Nobel Prize. Andy was enough of a biochemist...that was the time that a number of biochemists around the country (enzymologists) — Bernie Horecker at the National Institutes of Health...
- VM: Racker...
- CF: ...Efraim Racker — were working on transketolase and transaldolase, C_7 , C_5 , C_2 , moving around. Andy knew about this. I knew about it. And so Calvin would often, when he left the round table, would bring his staff together in the little glassed-in office there where there was a blackboard which there wasn't out around the round table in that lab. He'd spend a lot of time (Marilyn would go on)...he'd spend some time with us and that's where he would really — it was worse than the Friday sessions — he would really tear us apart. And talking about transketolase and transaldolase with Calvin didn't mean anything to him, or he wasn't quite sure how the C_2 s got in I repeated a minute ago. Andy finally said, I remember in exasperation, when he told about transketolase and there were C_7 s and C_5 s, and Andy popping his fist back equally to Calvin and it got very hot(?), not personally but very....and said, "Melvin, don't you realise that $C_5 + C_1 = C_6$, divided by 2 = C_3 and that gives you 2 molecules of phosphoglyceric acid, only one of which will be labelled. And you know

Martin Gibbs' work on asymmetric labelling." You could just see Calvin — it fell into place and at that point he disappeared, went back to his office. Two to three hours later he came back with the whole mechanism of the carboxylation of ribulose worked out and how it was an aldol transformation and so forth. That was the genius of the man. He could put it together like we couldn't.

VM: When Andy said $C_5 + C_1$, was it clear...did you all understand what he meant by C_5 ?

CF: Oh yes. Absolutely. We had seen ribulose diphosphate piling up and we knew...

VM: You knew it had to be ribulose diphosphate?

CF: Yes. But Calvin had the mechanism — we weren't sure about the mechanism and how you would do it and why you would get asymmetric labelling and all that sort of thing but Calvin had it all right down.

VM: What about the other playing around with these numbers in all the intermediate rearrangements in sedoheptulose and the xylulose and the erythrose?

CF: From there on it fell right together and it was primarily Andy and Calvin that put it together. The whole Path of Carbon is published here.

VM: Oh, yes, I realise that. I came in too late to have heard much of the early discussion about that. It was all pretty well accepted. But this was Andy and Calvin between them working out the permutations of transaldolase and transketolase and what they could do with it?

CF: Andy had to help Calvin with this, yes. Calvin would often come over to our glassed-in cage. I had a desk with Andy in there and that was the case. But this all started with collaboration with Arnon and enzymology. When Arnon had gotten this chloroplast reaction, we knew there was a ribulose diphosphate carboxylase enzyme. Calvin had worked this out. Arnon had been isolating chloroplasts and they were special chloroplasts and only Arnon could do this properly and so forth. And that bugged Calvin, I'm sure. From higher plants using spinach and Calvin had not done any higher plant work at that time, I don't think. So, by gosh, my contribution here was if you did all the photosynthesis then our new enzyme, which we had just submitted for publication (Rod Quayle, myself, Benson and Calvin), is going to be in that chloroplast. Could we go down to Dan Arnon's and get him to prepare chloroplasts with him? Feed them hot CO_2 and see...just a cell-free preparation, enzymatic reaction, put in the substrate, ribulose diphosphate which we eluted off chromatograms and turn on the lights.

VM: And you would have expected ribulose diphosphate...?

CF: I would have expected PGA.

VM: But you would have expected RuDP to get into the chloroplasts?

CF: Into the chloroplasts, yes.

VM: Whereas, of course, it wouldn't get into the cell.

CF: Exactly.

VM: So *he* wanted to do that experiment?

CF: No, *we* wanted to do that experiment.

VM: Oh, that was not his suggestion that you work with Arnon?

CF: No. But he said, “Well, if you want to waste your time, yes.” We told him about it and that we wanted to do it. That’s a direct quote. And then he turned around and walked away — typical. So, there were two other post-docs. who were good friends of mine. One of them was one of Van Neil’s students working with Arnon, Marybelle Allen, and Bob Whatley from Oxford.

VM: Yes, I’ve been in touch with him; we’ll be seeing him.

CF: Well, remind Bob of our visit, Andy’s and my visit down there. It was marvellous. We had the ribulose diphosphate in little vials, eluted off chromatograms, and we had hot CO₂ sealed up with a little syringe ready to shoot it in. And we went down to Arnon’s lab. to do this experiment. and Arnon had, you know, the white towels laid out all over the bench. Marybelle had the mortar and pestle and the acid-cleaned sand, and Andy and I were down there with the stuff ready to go. And Marybelle and Bob, the four of us, lined up when Arnon, in a pristine white coat and so forth, marched in with a tray full of spinach that had been kept in the cold room to be nice and crisp. And the procedure started. It was ceremonial, absolutely incredible. Marybelle and Bob dumped the leaves in the mortar and pestle, someone dumped the sand in, Arnon grabbed the pestle and ground the chloroplasts (*Editor: this should be “spinach”*), they went to the right centrifuge with the right speed with the right head on it, spun down the cell debris, they got beautiful dark green colours — pure chloroplasts. We suspended this in a nice buffer, a weak buffer because you don’t want to foul up chromatography, and covered everything up and it was done in the dark because this was an enzymatic reaction — we had a substrate and so forth — we shot it in over a period of time, took out samples and shot them with alcohol and killed it, took the killed things back up the hill and ran the chromatograms. Well, Melvin knew about this all right, that we had done it. “When are you going to develop those chromatograms, Clint and Andy?” I was working very close with Andy on this and: “Melvin, we know we only fixed 300 counts.” You know, we are doing a cell-free preparation and it was in the early days of cell biology, believe me. And we said, “It’s going to take two weeks, Melvin.”

VM: For the chromatogram to sit on film.

CF: Yep. So one day when Calvin came in Andy said, “Yep, it’s been two weeks. Let’s go up and develop the chromatogram.” So Andy and I and Melvin went up to the smelly room and Andy (*indecipherable*) took it out and clipped it onto the things and dipped it into the developer. And you know the white background on the film and you pull it up and down and keep looking and, in about a couple of minutes in the developer, up he pulled and there in the lower right-hand corner was a single spot just where phosphoglyceric acid showed. You’ve seen pictures of that chromatogram — famous. Melvin cheered before we even counted it, anything, and dashed out — we were afraid he was going to expose the film (*indecipherable*) but we had it in the fixer by then — and came back down to the lab. later with a short note prepared for a submission to the Journal of the American Chemical Society: Quayle — no, not Quayle; Benson, Fuller and Calvin, I believe, something like that. Maybe some other authors; I just don’t remember because it never got published that way. Not a mention of Arnon in whose lab. we had done the experiment).

VM: Go on. What happened? Did you guys agree to that?

CF: Well, we were sick. Al Bassham was involved with Andy and I. Al and I went over and had a long talk with Bert Tolbert, "What the hell do we do, Bert? Calvin wants to publish this without even recognising Arnon." So we called up Marybelle and Bob and said this was going to happen and they said, "Dan has done the same thing without mentioning Calvin." That's a true story. And so...

VM: But did Dan have...? Dan hadn't run chromatograms?

CF: No. We ran the chromatograms but he had put our names on it.

VM: Oh, I see.

CF: But it was just a draft; both of them were just drafts. Calvin gave us the written stuff with a blank space for tables so we could fill in the data and told Andy, Al and me to do that. We felt awful. I was scared to death. However, Bert was very good; he said, "You better go down and talk it over with...and have Marybelle and so forth talk it over with Dan and I'll talk it over with Calvin". He did. Calvin just showed up the next day and said, "I think I've found a better place to publish it and we'll take our time and it came out as one of the *Paths of Carbon*.

VM: Again without mentioning Arnon?

CF: Yeah, but he separated it out and put it as a separate piece of a large paper when...No, it was a paper. This involved the beginning of the departure of me from that lab. I am probably skipping over something. What he did was submit it without our knowledge, Andy's and mine, an abstract to the American Society of Plant Physiology was meeting in Gainesville, Florida the spring of '54. He had entitled that "The Enzymatic Carboxylation of Ribulose Diphosphate with Carboxylase to form Phosphoglyceric Acid," not mentioning too much about chloroplasts but using some steady-state data. Just an abstract, OK. And Andy and I saw this and we were both pretty annoyed because our names were on it and we hadn't even seen it. Calvin and I had had some brief discussions before because I essentially wanted to get on with my own career. I didn't want to become a member of Calvin's staff and I don't think he did, too. I was very roughshod and young and naive and pretty... Well, I didn't want to take that kind of environment without blowing it myself. I would react to Calvin like I shouldn't react to Calvin and so forth.

We pretty much had decided he would help me find a job. The next step in this, as a result of this paper, he said, "Clint, I can't possibly go as a plant physiologist to this meeting in Gainesville. Would you like to go and present this paper?" And I said I would be delighted to; both of us were. I was looking for a job and it was a good place for me to look. And there was nothing difficult about this. It was marvellous how Calvin could be very helpful and compassionate and I felt great about it. He said, "Don't your folks live...(this was the real humanist in Calvin)...don't your folks live in the Northeast somewhere?" And I said they lived in Providence, Rhode Island. And he said, "Why don't you just hop up from Florida to go to New England to visit on the way back to the lab?" He said "I know some people at Brookhaven National Laboratory and we can send you up there. Martin Gibbs is there and is doing all this nice asymmetric work and I'd like to have you talk to him and see what is going on." So on my way up I stopped to see Marty. It was the year of one of the hurricanes, the '54 hurricane, one of the worst hurricanes they ever had, and I was marooned at

Brookhaven and couldn't get home across the sand(?) to my parents. So I spent the weekend with Marty and had a great time. I spent the next day, on Monday, there and Marty had the head of the Biology Department call me in (whom he had talked to, apparently, on the phone). Marty wanted me to come to Brookhaven and they offered me a job and I got back to Berkeley, talked it over with Calvin and he said, "Great, Clint. You can launch your own career in microbial photosynthesis and I think that's wonderful."

VM: And you wanted to come back to the East Coast, as well, did you? Or that wasn't so important?

CF: I don't know. Sure it was important. I mean, we loved California, we really did...I don't know: it was a chance for me to go out on my own and do what I thought I could have done with the wonderful training I had had from George Kidder at Amherst and Ed Tatum at Stanford and Melvin Calvin at Berkeley. And Melvin, more than other people in a practical sense was after my Ph.D. when the pressures were off, really focused my career, which I spent up until 1986 purely in photosynthesis and microbial photosynthesis and published 150 papers in this area and got to be President of the International Society of Photosynthetic Prokaryotes, held the triennial meeting here at Amherst a number of years ago in 1991 with the biggest Russian delegation which ever came and eventually led, I think, to my being awarded an honorary degree which I am accepting next September in Moscow.

VM: Well, there are other things I would like to talk about now that we are talking about your career after you left Berkeley, let's complete it..

CF: That's how I left Berkeley to go back.

VM: OK, let's complete it. So you went to work with Marty Gibbs for a while at Brookhaven.

CF: Actually I had an independent appointment there but published a couple of papers with Marty. I was right next door to him. Those were the wonderful days. The Atomic Energy Commission, now the DOE, was running the lab.; they set me up with a technician, money for a post-doc. and all the equipment I could want. Calvin was very helpful in getting people interested to come to work with me and I had summer visitors, very distinguished ones — Brookhaven was a big programme — I was surrounded by people like Dan Koshland and Arthur Kornberg in the labs. upstairs.

VM: They were there at the time?

CF: Yes. Arthur Kornberg worked with Dan Koshland every summer there. I got to know him very well. He's going into the phosphate polymer business. That's full circle. He is very much interested in polyesters which are cellular accumulations with DNA and RNA phosphates. It's a long story but I talked to him recently on the phone about common work and so forth.

VM: When we talked at lunch you mentioned that Marty Gibbs' observation on asymmetric labelling in glucose was very important. Could you amplify why you thought that was the case?

CF: Well, if you were looking for a C_2 acceptor, a single one, which was Calvin's main drive and push, and you picked CO_2 on the end to...we were looking at things like glycine, glycolic acid, you'd end up with (*indecipherable*) C_3 in the carboxyl group

of phosphoglyceric acid. If that was put together you would find C_4 into a C_6 and C_3 equally labelled.

VM: That was their idea, wasn't it?

CF: Exactly. However, Marty Gibbs came along and found that at very early times, working with *Scenedesmus* and other things.... (this was when I was working not in Marty's lab. but next to him. He finished it then; he was working on it before I went there and that was why I was interested in getting there, I think)...that it was Bennett (or Dennett; spelling not clear] and Bernie Horecker was there, who was a great man on sugar degradations and transketolase, and he showed clearly that in early times that you recycle things and find C_6 , let's say, and 60% of the carbon would be in C_3 and 30% in C_4 and the rest of it scattered around. It was definitely not symmetrically labelled. But if you only labelled... if you labelled the ene-diol section 2 position of ribulose diphosphate and added carbon to that (of course the ene-diol reaction which Calvin came up with a brilliant mechanism of) then you'd find only the C_3 of the product labelled and the C_4 would not be labelled at zero time. So that's why it was important. OK? Is that clear?

VM: Yes, OK. There is another point that I wanted to ask you because you've mentioned it several times about people calling Melvin by his first name. When I got there in '56 no one was doing it, nobody at all. Andy, of course, had gone by then. You had gone by then. But you distinctly remember that you and Andy called him by his first name at that time?

CF: Oh, yes. And everyone in the lab. was, too.

VM: Everyone in the lab. was?

CF: I think so. Well, maybe people from Europe weren't. Jacques Mayaudon, I remember, always called him Professor Calvin. He was 40 years old and a relatively young guy and in American universities I always called my Ph.D. mentor by his first name and my undergraduate advisors. It is somewhat of a shock to me that times have changed and particularly our European colleagues. I like being called by my first name but I finally gave up because in the American university, after you get to be 65 years old, your undergraduates don't feel comfortable calling you by your first name.

VM: It is simply the fact that not very long after you left it was no longer the practice to do so. It must have ended with the people, your generation of people, who left at that time. It got resurrected many years later...

CF: In the 60s, yeah.

VM: ...when Mel Klein and I decided that this had gone on long enough and we must call him Melvin. But we noticed that at that time that we were the people who had not been his students and those who had, or close to his students, like Al and Dick and the others really felt uncomfortable calling him by his first name although now they do so. But it took them a long time and they weren't prepared...

CF: I guess I was brought up with it with Ed Tatum, who is just as distinguished as Melvin.

VM: So the other point I wanted to ask you, and I don't know whether you were there at the time and whether there is anything much you can add, but it has never really been clear to us why Andy left. Do you know why Andy left?

CF: The same reason I left. Andy — I know this for a fact because Andy and I had long, deep and heartfelt discussions about it — Melvin just for some reason wanted his staff to be his staff in a classical sense and he got some very brilliant people. It depends a lot on personality. Now Al Bassham, for instance, has a personality that just loved that. He loved the permanence, the exciting science he was doing, he was compromising with nothing.

VM: I must stop you there because the tape's about to run (*out*).

Tape turned over

CF: A lot of it was personality, too. I have compared Andy's and Al's personality and my own. My own was (*indecipherable*) even more extreme, perhaps. Andy was a brilliant, creative guy. He was interested in breadth, much broader than Calvin's interest in breadth, but he wasn't quite as good as Calvin and, therefore, he didn't have the group of 40 Ph.D.s working at it, and so forth. But he'd like to try, what I would call in a positive way, 'flighty' kinds of experiments on strange situations. Melvin didn't like that — "where was that leading Andy?", you know. And Andy liked to be independent, intellectually. He had some personal problems, which are really not relevant, but when one has personal problems it means a move sometimes is called for. That just was an additive factor, I'm sure. He had an offer at La Jolla which was just Andy's world. They were a bunch of dreamers down there and marine biologists were into all sorts of strange things. Andy got into the mango plants and the underwater business and he just leapt at it. It was about a year after I left, I think. He belonged independent, as I did, very cordially.

VM: Well, it happened again to other people and I was one of those people also who left for exactly those reasons and some others did. When you commented about Melvin's attitude not being the same as Andy's in terms of chasing off in all sorts of directions did Melvin actually try and stop Andy doing that or did he just wash his hands of him? What was his reaction when Andy did it? After all Andy was a grand man and the two...

CF: I think that deserves our attention (?). I never saw what I considered an important disagreement between Melvin and Andy that wasn't science. You know, that was Calvin's technique. I knew Dan Bradley also left for the same reason about the same time, as a matter of fact. I'd be dishonest if I said exactly why.

VM: I suppose at that time the carbon cycle and, therefore, Melvin's reputation, was not cut and dried and firmly established.

CF: That's right.

VM: It was on the border and Melvin, perhaps in the light of what you said earlier, was really very keen to get it wrapped up.

CF: I think he was, I know he was pushing very hard.

VM: And he wasn't terribly sympathetic with the idea of doing other things. Because once it got wrapped up, and there was a big final discussion in '58 which was the last of the

big arguments, around Otto Kandler and the labelling pattern. After that it clearly got very different and people then shot off — including Melvin — Melvin also shot off in all directions after that.

CF: Absolutely.

VM: But I see the point that some years earlier, when things were not so resolved, he was less keen to dilute effort.

CF: But he never held it against any of us. He just was so helpful and I just have so much respect for the man over the years. There is a marvellous story, getting back to Amherst, and maybe this would be a good place to end. Calvin visited Amherst several times before even Dick (*Goldsby*) was here. But before that, I'm trying to think exactly when it was. I was at Dartmouth; it was the middle '60s.

VM: Had you gone there? OK but we must come back to your career because we didn't finish it. You were at Dartmouth.

CF: We finished it at Calvin.

VM: Right; you were in Dartmouth.

CF: I was at Dartmouth Medical School sometime in the middle '60s; trying to remember the sixties. Anyway, Calvin had been invited to give, I believe, a Sigma Xi lecture at Amherst College. George Kidder was there at the time; he knew I was close associate of Calvin and asked me to come down, which I did. So the only connection with Amherst was Kidder. Kidder probably initiated the lecture before Blankenship and Dick Goldsby were around. I came down and he gave the lecture. It was very interesting because it was held in Johnson Chapel, which is a famous place. And Calvin always wanted to come back and give his other lectures, which he did, in Johnson Chapel. It is an old 18th century chapel on the Amherst campus, beautiful, with the white boxes, you know, all the colonial-type thing. He gave the lecture there and there were several members of the , there because it was called...the title of the lecture was called "Chemical Evolution and the Origin of Life", it was that period of Calvin's time. So Calvin gave his wonderful talk. If you've heard it you know how everything just fell into place and life, DNA, life was there. It was the early days of the DNA revolution, , of course and so...At the end of the lecture, during the question period, one of the members of the clerics stood up and the President of Amherst College, who was chairing the session, pointed to him and said, "Father so and so" and the gentleman of the cloth said, "Professor Calvin, you've given a wonderful lecture and it's marvellous but as a man of God tell me, what have you left for God to do?" And Calvin, in an instant flash: "God sent me here to tell you about it." (*Laughter*)

VM: And how did your cleric react?

CF: There was just this deadly silence. He stunned everybody and then tremendous laughter and cheers. Ego, ego, ego.

VM: To backtrack, I'm sorry, let's come back to your career. We left you with Marty Gibbs...not with Marty Gibbs but with the company with Marty Gibbs...

CF: Then I had the beginning of my English experience. I had a...I got a National Science Foundation...Brookhaven National Lab. was run by a bunch of universities and they

had a sabbatical system. I had been there six years and so I applied for a National Science Foundation so-called Senior Fellowship (I was 30 something at the time), Senior Fellowship award which would pay for you and your family to go across the ocean in the big boats in those days (I was 40, I guess, and child care wasn't as well known) but anyway I had known Hans Kornberg, of course, at Berkeley, and Rod Quayle at Berkeley, they were both at Oxford and they somehow or other talked Krebs into sponsoring my fellowship. So I arrived at Oxford *en famille* on the boat, *SS United States* at...dear, my geography fails me...

VM: Southampton, not Dover, nor for those...

CF: Southampton, and was met by my little red Hillman Minx station wagon, and the driver jammed four kids in the back seat and it came with a luggage rack and off we went to Oxford. We had a wonderful experience. We lived in Queen's College Playing Fields which was the old leper hospital outside the walls of Oxford, a 13th century building that had been made over into a residence. And we had a wonderful year with Krebs and Krebs' lab. We did a grand tour of Europe with all our children. Had a wonderful English story to end the grand tour. When we went back from Norway, from Bergen, Norway, we took the boat to England and one of my kids got sick when we were in a lovely place called Gol in Norway and we suddenly discovered he was covered with spots. We didn't know what it was except we remembered that in school in Oxford (he was in a pre-school or something — he was four years old), there had been a lot of chicken pox. And we had other kids and thought, oh my God. Anyway we got in the car and drove to Bergen where we had a reservation and we stayed there for three or four days while he got over the problem. But he was a mess, a scabby mess. But we had to get back and he was no longer contagious and no longer had a fever so we made reservations on the boat to go to Newcastle, overnight, I think it was as I recall. And we got on the boat which was loaded with English tourists going back to Norway (*Editor: presumably this should be "going back to England"*) and we were standing in line, and as you might image, you don't know my wife, but we have four very, very Norwegian blond, blue-eyed kids.

VM: Is your wife Norwegian?

CF: No, but I meant that we were in Norway at the time and they looked very Scandinavian. This very nice chap was in line with my wife — she had two kids, I had two kids in another line, something like that was the way we worked it, but she had Jonathan along with her with his scabby appearance. And this chap said, "Oh, are you from Norway going back to England?" And she said, "Oh, no, we're Americans and we're just on our way back to England because I have a child who got sick." And the chap looked at him. And my wife said, "Well, he's fine now but did have an awful bout with small pox." And the chap just looked at him, looked at her, looked at him. My wife, of course, had completely misspoken. She had meant chicken pox. And he said, "My, I didn't know they had that in Norway." And she said, "Oh, no. He got it in England." (*Laughter*) There was sort of an empty spot in line. Carol said, "This man was so strange." When she told me what she had said, I said "Oh my God" and dashed over and told him it was just chicken pox and he said, "Well, I thought it must be something else!"

So we got back to Krebs' lab. and spent the rest of the year there. I came back to the United States (*indecipherable*) but while I was still in Oxford I got a telegram from a chap in Hanover, New Hampshire, who signed the telegram "Dean of Dartmouth Medical School." My first reaction was that Dartmouth doesn't have a medical

school. But he asked if I would be interested in becoming Chairman of the Microbiology Department. You see, I would become a microbiologist again. And I said, "Of course." I was looking for an academic position so he flew me back from Oxford and interviewed me and one thing led to another and he offered me the chairmanship at Dartmouth Medical School, which was in crisis because it was being disaccredited by the AMA and they had to shape up and they hired six new department heads, which I was one of...

VM: That was in the early '60s?

CF: That was the year after Krebs, '61 or '62.

VM: How long did you stay there?

CF: I stayed there through 1967 when I had another sabbatical. I went to the University of California at Riverside to do a sabbatical and during that period of time...yes, during that period of time, I got another approach while I was at Riverside — would I be interested in coming to the University of Tennessee in Oak Ridge where they were setting up a new graduate school for biomedical sciences at Oak Ridge National Laboratory, which the University would have but this would be a graduate school because the then chairman of the Atomic Energy Commission, Glenn Seaborg, had chided the people on the Atomic Energy Commission, including Alvin Weinberg, who was head of the Oak Ridge National Lab., that you people at the unex of science, you use it all but you don't produce any of your own. And that led to the University setting up this graduate school to produce Ph.D.s at Oak Ridge, which I was the Director of for five years or so and the next step out of the blue I was offered...that was the best of all worlds, except for living conditions in Oak Ridge. I had the best of my National Lab. advantage, a faculty of 100 distinguished people to start a new school with and so forth. But living in the Southeast, in red neck country of Tennessee was not for my children and me, and I got an offer as the Chairman of the Biochemistry Department, a new Biochemistry Department in building here at the U. Mass.

VM: When was that?

SM: '73?

CF: No, it was 1971-72.

VM: And you stayed here for the rest of your career until you retired, you said, in 199...?

CF: 1991. And now I have a full-time active research lab. in polymer science and engineering that originated back in Melvin's influence again, splendid, in Berkeley.

VM: One last topic to finish this up. This question of ORL. Everybody, you know the argument, everybody who lived in ORL thought it was a special place.

CF: It was.

VM: Why was it a special place?

CF: You know atomic energy had its initial cyclotrons there and so forth and we were very well aware of that because we could read the background. But that, in itself, is sort of special when you know the whole Lawrence story and it meant a lot to us in

those days. It had its origin in atomic energy and that was a very big thing — the bomb — to our generation and the generation of the young people who were there were very much World War II products and, you know, we survived by not going to Japan, and the bomb and all the mixed feelings people have about that way to end the war. I had to say it, I wasn't too mixed...

VM: Do you think the building, do you think the structure of the building, was an important factor in the way the group operated?

CF: Yes. It was so cramped, everyone was thrown together and the design of the Calvin Building, of course, arose from that and, I guess, from the set-up in LSB, too. And the discipline of Calvin's lab. was the building, the physical building fitted his *modus vivendi*, which became our *modus vivendi* whether we liked it or not if we were there for the long term or not. And just getting together in the lab. where you had a bench coming out and we were working and Calvin would come in with Marilyn and you were right there, the table was right there, and you would drop everything if you could and you would surround...and that would be it. Our offices, either at our bench or our visitors' were at the benches, and Andy and I and Al had a shared office in the glassed-in area and we were stumbling all over each other. And the people there — I might to ask — it was a mixture, an international mixture — and everyone knew that building, from Japan, to Germany to England, they knew what it was for, they came knowing what it was and they fit into the spirit of the structure, almost. I sound a bit mystic but I think we all came away feeling, it's unanimous, we all came away feeling that way and I know down in the pits where we stored the chromatograms, that awful place — we used to go down there — and then just how horrible it was. Up in that chromatography room and the background counts from the old...were frightening. But also the spirit of adventure, too, that was part of it.

VM: Yes, it was partly that. Clearly, the people and the youthfulness of the people and the sense of unity in the group.

CF: And the building was all part of that because it was a tough time in the '40s and '50's. It was after the war and nothing like you people suffered, but nevertheless jobs were difficult to get.

VM: So, last, last thing, how well do you think the new Round Building recreated it in a modern sense?

CF: Well, my one visit both there to Melvin's office and to the whole three stories of the round structure — I don't think it ever can really replace...It works, it's Melvin's system, it works beautifully.

VM: It worked while he was there. It doesn't work any longer.

CF: Well, right. That's because it was designed around him. But, I think Melvin perhaps made a mistake there. He couldn't redesign the intimacy and the spirit of adventure and the things you had to do to put up with that damn building sometimes. I don't know, just standing out on the steps with our deerstalkers on brings back a lot of memories. Those steps were important.

VM: OK, well I think you have other things to do...

CF: It's your pleasure but I think we can all think of things but they can't all be there.

VM: We are most grateful to you for a very entertaining hour and again helping to complete the story.

CF: Well, I am most anxious to see what comes out this. And I certainly said all the things...I got the God story in and that's so typically Calvin.

VM: OK. Thanks a lot.

CF: Wonderful to see you and now that I'm free to travel again...I have another colleague, a very close recent colleague...I have many colleagues in England but someday I hope to see Rod Quayle again. I saw him in Atlanta; he came to a meeting in Atlanta a number of years ago as an invited speaker and Otto Kandler was there at the same time, so I see these people.

VM: Well, he's alive and well, and we've seen Rod and we're going to see Malcolm Thain who's another guy with a deerstalker hat.

CF: Say you talked to me.

VM: And remember your promise: you are going to send us a picture of you in the deerstalker.

CF: I've got it down on my piece of paper and that's pretty good.

VM: OK. Thanks a lot.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name R. CLINTON FULLER
Date of birth 5 MARCH 1925 Birthplace Providence, R.I.
Father's full name RUFUS C FULLER JR
Occupation PROFESSOR Birthplace Providence R.I.
Mother's full name Alice Anthony Fuller
Occupation At Home Birthplace Providence R.I.
Your spouse CAROL S. FULLER
Occupation AT HOME Birthplace Ansonia, Conn.
Your children David, Katherine, Lynn, Jonathan
Where did you grow up? Providence R.I.
Present community Amherst, MASS.
Education B.S., M.S., PH.D.
Occupation(s) Research Scientist, Professor
Administrator
Areas of expertise Photosynthesis and Biopolymers
in general. Cell Biochemistry
Other interests or activities Wide range
Organizations in which you are active Scientific Societies

Chapter 33

SIR HANS L. KORNBERG

Boston, Massachusetts

July 26th, 1996

VM = Vivian Moses; HK = Hans Kornberg

VM: This is a conversation with Hans Kornberg in Boston on the 26th of July, 1996.

Hans, what was your background at the time when you first went to Calvin and why did you go there?

HK: Well, I first met Calvin when he attended a conference in Sheffield which had been convened by my dear teacher and friend, Hans Krebs, in 1949 or 1950. At the time I was just a beginning graduate student — it must have been 1949 because Calvin was then on his way to the first Congress of Biochemistry in Cambridge. As part of the Sheffield meeting, Hans Krebs had commissioned me to take photographs of all the notable participants and I took numerous photographs of Melvin Calvin and it was on the basis of this, and of hearing his paper at Cambridge, that I decided he was a man I would very much like to work with, albeit briefly. In 1953 I was awarded a Commonwealth Fund Fellowship and found myself working, for the first of the two years of the Fellowship, in the laboratory of Professor Efraim Racker at Yale. Racker decided that one year at Yale, or at least *that* year at Yale was sufficient and he would really like to return to New York, where he had been working for many years, so he accepted, in 1954, the position as Director of one of the divisions of the Public Health Research Institute of the City of New York, Inc. My Fellowship allowed me, no required me to travel for at least one and preferably more months between years one and two, and given the option of either travelling and seeing the United States at the expense of the Commonwealth Fund or lugging bottles and lab. equipment from New Haven to New York, I naturally chose the latter (*Editor: he surely means "former"!*). So I wrote to Calvin saying that I would be passing in the direction of California in May of 1954 and could I possibly spend some time working in his laboratory to learn these new and exciting techniques in return for which I would be prepared to offer my new-found expertise as an enzymologist and perhaps enable him to decide the question which he had posed publicly, namely, whether the cleavage by CO₂ of ribulose *bis*phosphate should lead to two molecules of 3-phosphoglyceric acid or one of 3-phosphoglyceric acid and one of glyceraldehyde-3-phosphate with the input of reducing power. And I said, "Well, you know, vi haf vays of finding out using enzymes." And he said, "Well, anybody who knows enzymes must be terrific and, you know, useful, so, welcome!" So he then went to one of his young post-doctorals, Professor, oh then Dr. J. Rodney Quayle, and said, "Clear a bench. Kornberg is

coming.” And that meant, of course, in everybody’s mind, that it was Arthur Kornberg. And I suspect also in Melvin’s mind (*laughter*) because when I appeared there was a little frisson of non-recognition as it was quite clear that I was not Arthur Kornberg. As I always made clear to everybody, including Arthur Kornberg, although we are often confused by name nobody who has ever seen us could possibly confuse us because he is a man of medium stature with receding hair and glasses! (*Laughter: Hans Kornberg, of course, is exactly the same!*) Anyway, I duly appeared and Rod Quayle, who was, of course, in awe of this great biochemist descending on us (*them?*), also gulped and then decided that, well, maybe he could teach me something.

VM: Of course you didn’t know Rod at that time.

HK: Not at all. Rod Quayle, at that time, was a man who had had an interest in insect pigments and had somehow been deflected from this on coming to Calvin’s lab. but was quite willing to teach me about the chromatography if I would teach him about ammonium sulphate precipitation and what NADH stood for and glyceraldehyde-3-phosphate and so on. So that is how we started a very happy collaboration. Of course, it was a marvellous introduction to a group which was inspired, I think, by a common passion for solving the problem but had an enormous range of approaches to tackling that problem. It was not free of amusing episodes in this, some of which were, in fact, slightly embarrassing. Like Melvin’s tendency to come in, demand to know the results of an experiment he had suggested and then going off to give a high-powered seminar somewhere in which he announced the tentative speculations I had advanced as established fact. It was this, of course, which led, of course, to the famous incident of the sequence, which you already know about.

VM: Which you will tell us, anyway, because this is a record...

HK: Right. Well, what happened was that Melvin would come in, as I said, and demand to know what the experiment showed and whether or not it was glyceraldehyde-3-phosphate or 3-phosphoglyceric, and I would say “Well, you know the evidence is strongly in favour of ruling out glyceraldehyde-3-phosphate but I need to run some more controls and I need to do this and I need to do that.” And he would gradually shed clothes, as you know, when he would talk to you. I finally managed to get him down to his vest on one occasion when we argued about, you know, the significance of the results and what still needs to be done. But anyway; I was then rung up from a colleague who had heard Melvin talk in, I think it was New York or it may have been St. Louis, who said, “Gee, that’s great work you are doing. I have just been hearing about your definitive ruling out of the (*Editor: not clear; glyceraldehyde-3-phosphate?*)” Of course, my hair (in those days I still had hair), stood on end at this point because I thought, I’ve done nothing of the kind. And so when Melvin came back, I said, “Look, Melvin,” (and publicly because everyone was listening), I said, “Melvin, I think it would be better if I did not discuss on-going experiments with you until I am in a position to tell you which way they go.” And all the boys around me sort of nodded and nudged each other and said, “Right, you know, we’ll get him for this”. I was living as an inmate of I-House at the time and after my honest day’s work I had gone to a party that evening and had gone to bed at about midnight and about two in the morning I was woken up by a thunderous knocking at the door and there were two of what appeared to be armed guards outside who were actually campus policemen, or actually Atomic Energy Authority policemen, who handed me a note which said, “Security violation. Notebook marked ‘secret’ left open on desk.” What apparently had happened was that the boys had gone to the safe and had got out the purple stamp marked ‘Secret’ and had marked the first page of my notebook ‘Secret’ because I wouldn’t tell Calvin the results. Since I had no security clearance to be

working in the Atomic Energy Laboratory in any case, I immediately had visions of myself either in handcuffs or in chains or on Ellis Island or deported, so I went to see Nate Tolbert, who was then the Acting Director because Calvin, fortunately, was away.

VM: Bert Tolbert.

HK: Bert Tolbert. I beg your pardon. Thank you. Nate was his brother. And Bert said, having listened to my story, he said "Oh, don't worry. We'll clear this up." So he talked to whoever was the security chief up in the laboratory on the Hill, we went up there, and we sat down, me quaking in my boots, and the security guy said, "Well, this is a very, very serious matter." And Bert said, "Well, it is all a trivial mistake and a joke and so on, could we just not forget it." "Oh, no," he said, "a file has been opened and if a file has been opened we have to follow procedure and this will have to go to Washington." And I thought, oh, my gosh! And Bert said, "Well, just to be clear, could we possibly just see the evidence on which you are taking action?" And so the man said, "Well, yes, here it is. It is a notebook with on the front page a big stamp saying 'Secret' which clearly ought not to have been left out of the safe." And Bert took this notebook and immediately ripped out the front page and he said, "I don't see any stamp." And that was the end of that. (*Laughter*) And I returned.

VM: And you never knew who did it?

HK: I suspected. Anyway, my return back to the land of the living, as it were, was greeted with the usual applause and, of course, a party; we had a party that evening. And we went off camping in Yosemite and we did all sorts of jolly things that one did. So when I left to go back for the second half of my trip across the country...

VM: To go back to Racker?

HK: To Racker, but this time in New York, I was touched but not at all surprised that my by this time bosom friends should wish to give me farewell presents and these farewell presents included all sorts of goodies like reading matter for the journey home, all sorts of paperbacks and books of various kinds. And I gratefully accepted these and packed them away and that was it. When I finally returned to England, I went on board ship and again there was a farewell party and then we set off on the good ship the *Flandre* to England and I was sitting on the deck deciding that I ought to read some of these books which I had been given and I picked one and opened it and this time, inside, there was a red stamp which said 'This document contains material vital to the security of the United States and should on no account be, etc., etc.' And I thought, "Agh," and I tossed it overboard and picked up the next book which had an identical stamp — every one of these books had the same stamp in it! Fortunately, I managed to get rid of all except one, which I've kept. I thought to myself, you know, if this had become public knowledge I might not be sitting here where I'm sitting now.

VM: It's not in this very room, is it; this book?

HK: This book? No, alas, no, not here. But anyway it is one of the many pleasant memories of that time.

VM: When you first got there, you started by talking to Calvin about what you were going to do?

HK: Yes, indeed.

VM: To him alone?

HK: Talking to is perhaps too strong a word. Listening to, and then trying to get a word in edgewise.

VM: He knew exactly what he thought you ought to be doing?

HK: Well, he certainly did. I had explained to him the power and utility of enzymology as a means of picking up intermediates even in small amounts with cell-free extracts of *Chlorella*. Not that I had ever used *Chlorella* or knew how to break them up or done anything other than, you know, talking off the top of my head. But he was bowled over by that approach because he was basically a chemist and he had a chemist's view of how nature operated. "Bio-Organic Chemistry" was the name of the group, after all, which seemed to be a contradiction in terms; but however, oddly still tautologous since originally organic chemistry was defined as the chemistry of organised or living matter so it didn't need "bio" as well. But anyway, the thought that there were enzymes involved which had to act in concert and were regulated was not one which was very current in that group and, indeed, most of the people there were, I think, non-enzymologically contaminated.

VM: How were you going to show the presence...the formation, direct or...Let me rephrase it: how were you going to show whether or not glyceraldehyde phosphate was present?

HK: Well, there were two ways I was planning to do it, if I remember now. I am now talking about 1954 which is 42 years ago. I was going to see if we could trap glyceraldehyde-3-phosphate, if it were formed, either by doing the thing in the presence of arsenite and we did, indeed, use arsenate as a replacement for phosphate, and show the formation of, you know, the usual triosephosphate dehydrogenase reaction; or I was going to use the fact that it was in equilibrium with dihydroxyacetone phosphate and reduce it. And there we ran into trouble because there was a very active NADH oxidase system in the crude preparations we used but I found that by eliminating membrane fragments I could get rid of the NADH oxidase and then putting in control glyceraldehyde-3-phosphate (well, it wasn't actually; it was actually fructose-1,6-bisphosphate) and hoping that there was enough aldolase and triosephosphate isomerase present and α -glycerophosphate dehydrogenase I could show that it really did work and that it was dependent on fructose-1,6-bisphosphate and that there wasn't NADH oxidation. And this was done in a very crude, manually operated, Beckman spectrophotometer because there was no such thing as...I mean, we had recording spectrophotometers but these recording spectrophotometers were used for extremely chemical purposes and were not adapted for biological use. But we could show that if there were glyceraldehyde-3-phosphate present in fractions of a micromole, I would have been able to pick it up and I didn't. So that gave me confidence with both tests, both with the arsenate procedure and with the DHAP dehydrogenase procedure, to believe the primary cleavage was, in fact, two molecules of 3PGA and not glyceraldehyde-3-phosphate.

VM: Were you there when he, or whoever else might have done so, formulated the proposed mechanism for this carboxylation and split? For the carboxylation of...

HK: It had already been done by the time I'd arrived. The *Path XXI* paper had already appeared and also the famous paper which had the chromatogram of phosphate sugars labelled — do you remember that?

VM: Well, I don't know about *the* famous one. There were so many which had phosphate sugars labelled.

HK: The one which I remember is the one which had spots labelled fructose-P, glucose-P and Godnose-P.

VM: Godnose-P! I don't remember that one; I'll look up that one.

HK: So that had been done and there was a delightful man called Wilson...

VM: Alex Wilson.

HK: Alex Wilson, who was a New Zealander, who was largely responsible for the Godnose-P identification, I think. Altogether was a very lively character. I remember, also, Malcolm Thain and that is another part of the story because Rod Quayle and I, and his wife Yvonne, became very, very close friends as a result of our working together and, indeed, we went on trips together to Yosemite...

VM: May I ask you, were you married at the time?

HK: No I wasn't. No, no, I was very much not married at the time. And we went on trips together and so on and so on. And then I met a young lady when I, myself, had gone back to England and was working in Krebs' laboratory for one year only (although it turned out later to be six) and as a reward for my baby-sitting for a week for some visiting Americans, these visiting Americans took both of us to the theatre to see Beckett's *Waiting for Godot* and who should be sitting in front of us but Rod Quayle and his wife, Yvonne. And they had, Rod had come back to England and the only job that was open to him was in the Tropical Products Laboratory in which Michael (*Editor: should be Malcolm*) Thain, indeed, was one of the senior people. Rod did not find this very congenial; he didn't like the Civil Service atmosphere, he didn't like the type of work he was being offered and he was clearly unhappy. So when I went back to Oxford, I went to see Krebs and at that time my work had just stumbled onto something which later turned out...we had just published the glyoxylate cycle. And I mentioned to Krebs that the desirability, nay the necessity, of establishing that this cycle, which had been worked out largely on the basis of enzymology and simple experiments with microorganisms, that this ought to be put to the test by measuring the distribution of isotopes from labelled acetate in components of the Krebs cycle or in amino acids derived therefrom because it was quite clear that the glyoxylate cycle should lead to a distribution very different from that of the Krebs cycle and if both were operating then some intermediates should be done.

VM: You remember, of course, the work of the Carnegie group had been very active in the Krebs cycle?

HK: Indeed. And I modelled myself very much on that and got to know Phil Abelson quite well through that. Well, the upshot was that I told Krebs that I was neither sufficiently adept as a chemist nor knowledgeable in isotopery to be able to do this on my own but by very happy coincidence there was a man who was and Krebs immediately said, "Well, arrange for him to have a fellowship." And so Rod Quayle and I worked on opposite sides of the bench very happily for a number of years until he finally took off

and became a Professor of Microbiology in his own right and later, of course, a Vice Chancellor.

VM: When you were in Berkeley you worked in the Old Radiation Lab.?

HK: Indeed, the old wooden building.

VM: Were you in the main lab., in the big lab., or pushed away in a corner somewhere?

HK: No, I was in the main lab. In fact, Rod and I shared a bench and we were in the main lab. Memories are dim, but I do remember that it had a number of delightful features not the least of which was that if you smoked, and I did smoke, furiously, in the wrong place the fire alarms would go off and the next thing you'd know is that the benches would magically open where you thought there would be a cupboard containing glassware and out would pop a fireman with helmet and all and dragging hose after him. The first time I encountered this phenomenon I could hardly believe it.

VM: How do you mean the benches would open? Where was the fireman actually coming from?

HK: The building, was I think, partly on stilts and they went underneath and then climbed up...It was a matter of some astonishment to me.

VM: This happened to you actually while you were...

HK: This happened to me on one occasion, yes. I made myself very scarce in case I was asked for an explanation of how this visitation came to be about.

VM: When you were there, was Andy still there or had he gone?

HK: Oh, no, very much so. Andy was really the CEO of the lab. Melvin came in as *deus ex machina* but Andy Benson and Al Bassham were the two people who really ran it. And then there were people with special expertise like Rod with his chemistry and Clint Fuller with his botany and so on, but it was Andy who was, without doubt, the genius of the place, and listening very carefully to how his mind worked and how he explained things and how he devised tactics rather than strategy, it was clear that the inspiration and the realisation that the formation of a C_3 compound from C_1 was not $C_1 + C_2$ but $2 \times C_3$ arising from $C_1 + C_5$. That was very largely Andy Benson's. Looking back over the history of the thing, I am increasingly convinced that Andy should have, by rights, shared in the Nobel Prize for that work.

VM: That was also the time, approximately, I don't know if precisely, when the other rearrangements in the cycle were being considered — the C_3 s and C_4 s and C_7 s and so on, which was surely relevant to the work you were doing with Racker.

HK: Well, indeed. This was another reason why Calvin was anxious to offer me a place because I had been working in Racker's laboratory on transketolase. My friend, Paul Srere, at the next bench, was working on transaldolase. So between us we managed to combine some experience in shuttling C_4 , C_3 and C_5 s around. And we could, therefore write down two schemes, both of which would have led to ribulose *bis*phosphate, one involving aldolase and one not involving aldolase. In a later book that Krebs and I published in 1959, called *Energy Transformation in Living Matter*...

VM: '49? Must have been '59?

- HK:** I mean '59, I beg your pardon: '59...we put forward both schemes and said there was no evidence to decide between them and they may, in fact, be a mixture of both.
- VM:** When you were in Berkeley, then, was this a lively item of discussion — what these rearrangements were?
- HK:** It wasn't. Occasionally Dan Arnon and his group, particularly Bob Whatley, who was amongst them and whom I still see occasionally and still regard as a good friend, would come over, but unfortunately, for reasons of personal chemistry, the visits engendered more heat than light and I don't think...I mean Dan Arnon and Mel Calvin were like oil and water and it was clear that we were not going to get any further by discussing these. And I think Dan Arnon, to do him credit, was probably more receptive to the possible involvement of these enzymatic transformations than was the Calvin group.
- VM:** Arnon actually came into the building, did he, and discussed with...?
- HK:** I saw him...this is how I came to know him better because he had also attended the International Congress of Biochemistry in 1949 and had also visited Sheffield and I visited him in his home. But it is clear that he came in only when Calvin was not there, at least in the period that I was there. But Bob Whatley came very frequently.
- VM:** We are going to see Bob Whatley in Oxford.
- HK:** He was a neutral messenger.
- VM:** Yes. You mentioned some of the people who were there at the time — Al and Andy, Alex Wilson and Rod — do you remember others who were there at the time?
- HK:** I remember some people from the Donner Laboratory. I have already mentioned Bert Tolbert, and Dick Lemmon was another person whom I became quite friendly with. There was also Dr. Stanley, of course.
- VM:** Wendell Stanley.
- HK:** Wendell Stanley, yes.
- VM:** But he wasn't part of Calvin's group...
- HK:** No, he wasn't.
- VM:** He was in the Virus Lab.
- HK:** Very much so. I remember also there was a rather peculiar failure of identity there, or recognition of identity, because when I came in to be introduced to him — Dick Lemmon, I think it was who had catalysed this meeting — magically all doors were open (I was only a post-doc., damn it) and he came out beaming, with his hand out straight and said, "Dr. Krebs, Dr. Krebs, I'm delighted to meet you." And I grasped his hand firmly; I said, "Dr. Livingstone, I presume." (*Laughter*) I can happen.
- VM:** Did you know two ladies, one of them was Lorel Kay who was...
- HK:** I knew her, yes.

VM: degrading sedoheptulose, I think, and there was somebody else whom I can't remember...

HK: I think I met her but she hasn't made a very deep impression.

VM: All of those sugar degradations had really antedated your visit...

HK: Yes, yes.

VM: ...and the cycle was really being put together in its final form, I suppose, at that stage.

HK: Well, *Path XXI* had just appeared, I think, and that gave the whole cycle. The other work which led to it had been done before I came.

VM: And your activities really represented, perhaps, the first detailed investigation into the carboxylation mechanism itself.

HK: Probably, yes, because at that time there were all sorts of schemes (*which*) had been drawn up on how the carboxylation might occur. It was known that somehow the CO₂ carbon inserted itself between what was two and three on the ribose (*ribulose*) but there were all sorts of chemical compounds with double bonds there were postulated for which there was very little evidence. And some of these were actually tried and didn't work, of course.

VM: In retrospect...

HK: Hamamelonic acid. I remember now. Gosh; I haven't thought of that for 40 years!

VM: ...a favourite compound.

HK: Yes, that's right.

VM: Hamamelonic acid is not something one lies awake at night dreaming about.

HK: No, it's one of the...one of the great...It always reminds me of Christopher Fry's comment in one of his plays when the dead heroine mourns her young husband taken from life untimely and says, "How sad to be a coming man already gone." I think that is the state of hamamelonic acid, too.

VM: Looking back on it after so many years, what's your view about Calvin's lab. and labs. like that?

HK: Calvin obviously had a tremendous dynamism which swept you along. He was blessed by having a very happy home life. His wife was absolutely delightful, very firm with him but a wonderful hostess. He had the ability, also, to inspire not only loyalty but also affection in all with whom he came into contact. I mean, you could dislike him or at least you could fear him. He could be extremely fierce. I remember on those awful Friday mornings when we started off with a seminar at 8:00 am. I was leading a hectic social life so I believed that little good could be expected of any day we started by getting out of bed and to start at eight o'clock was just impossible. And you appeared and everybody would be sitting there quivering with apprehension because you didn't know who was going to be asked to speak. You know, he would look...this eye, this cold eye, would look around and he would say, "Hans, would you

care to tell us what you are doing?" And the answer was, "You'd better!" He inspired awe, fear, respect, admiration for his quickness of mind because I've never known a man who was quite so able to respond to novel ideas, assimilate them and then play with them and see the outcome as Calvin did. And although this all sounds negative, in fact he inspired in no one that I have met who worked with him anything other than affection and admiration. I really came to like him immensely.

VM: Do you think that he was really the whole of the group, the whole guiding force of the group?

HK: I think it is like saying, "Was Beecham the whole of the London Philharmonic Orchestra?" Without the orchestra he would have been nowhere. He would have been beating his arms in the air. I think it was Andy Benson and Al Bassham who provided the string section and the brass section and the percussion and everything that made it go. I think they were the leaders of the orchestra; he was the conductor.

VM: Have you seen other groups like that?

HK: This is a group which he directed. Now I have been in many other groups. I have been in Racker's group, I have been in Krebs' group. I've had groups of my own, as you have. But none of those have been directed. They usually have been people suggesting certain things and being there as, perhaps, courts of last appeal or perhaps of people who might suggest a directions, but they haven't been directed. Calvin directed. He actually came in and directed. Now, funnily enough, Krebs did the same thing to his immediate group, that is, he would come in and individually design the protocols of today's experiment with Len Eggleston or Reg Hems but he didn't do that to me because he didn't know anything about enzymology and he didn't know anything about microbiology, and he distrusted both of them.

VM: Those people with whom he did do it — were they technicians or independent scientists?

HK: They were technicians who became independent scientists. I mean, for example, the Professor of Biochemistry at Sheffield who succeeded Krebs at two removes, Walter Bartley, was his technician. David Hughes, who became Professor of Microbiology at Cardiff, was his technician and that goes on.

VM: Yes, I remember, you were his technician.

HK: Well, I was his technician for one year only. I was his technician ...This is how I got into biochemistry was by applying for the post of junior technician at 30 shillings a week rising to 35 when I left school because I didn't know what I wanted to do and I was summoned for interview and because of the remorseless intelligence of my answers, of course, I was appointed for the job. It was only later in life that I discovered that another contributory factor might have been that I was the only applicant!

VM: I think I can conclude that, at least in your view, Calvin's group was a rather remarkable and unique activity.

HK: It was unique in my experience. I have never known a group of people from such a wide background who could work together as happily. If one had to draw an analogy, it is not an analogy with which I am personally familiar but I am personally familiar

with people who are personally familiar with this. It is the people who coped with breaking the Enigma code during the war, which is the only...

VM: Well, it has struck me that there are several analogies with wartime experience and because that was an AEC-funded activity perhaps their own focused wartime activities in the military sphere would have primed them for that type of organisation.

HK: Yes, it could very well be, yes.

VM: Two last points, I think, before we finish. One of them is your view of the significance that the building itself, ORL, might have had in the way the group organised itself and the interactions that went on. Did that strike you at the time...?

HK: It didn't strike me at the time. In fact, I found on my subsequent visit to Berkeley, when the round building had been built, I felt the atmosphere had deteriorated. There wasn't this awful...there wasn't this delightful intimacy that there had been before. People had been literally compartmented. Somebody, I can't remember who, it may have been Krebs or somebody even older than that, once said that you could divide a scientist's career into three. The first is when he does his important work in intolerable conditions; the second is when he spends all his time designing a new building; the third is when he shows visitors round it. And I think the inception of the round building also marked, I think, the departure from the high point of Calvin's group's activities. It became much more pedestrian.

VM: Interesting. That quotation appeared on Al's old glassed-in office door or wall after he came back from Oxford. He might well have heard it from Krebs at that time.

HK: And, of course, Al and I overlapped at Oxford.

VM: Indeed. And the very last thing, although you mentioned that when you left Berkeley, of course, you went back to Racker and then you went back to Oxford. When you were in Oxford: Krebs had moved by that time?

HK: Krebs, in fact, came to visit me at Yale and this, again, is typical of Krebs. I mean, I had been a student in his department (not working directly with him but with R.E. Davis) and yet he had said that I could come back for one year in the first instance to him when I came back to England but he found it necessary to come to Yale in order ask me whether I would agree to his moving from Sheffield to Oxford. Which I thought was quite extraordinary.

VM: Whether you would agree to it?

HK: Whether I would agree to Krebs accepting the Chair at Oxford. Would it bother me if we were not to be in Sheffield? I thought it was remarkably courteous but it totally left me baffled that he sought my advice. I was then all of, what, 25?

VM: I remember when you were all of 25. In the pictures of you taken in the Berkeley group at the time, you were a much more dapper dresser than all the others.

HK: Not Calvin. Calvin was the dapperest of them all.

VM: Was he?

HK: He always had a fresh rosebud in his buttonhole. Always.

- VM: He did, that's right. But you, you came up in these pictures in a white suit or a...
- HK: Not a white suit. I could never resist a bargain and I bought some tropical suits and of course they were ridiculous, absolutely ridiculous. But I had them so I wore them.
- VM: All the others were in their typical sawed-off jeans...
- HK: Yes. This was the pre-jeans era.
- VM: Well, whatever it was when they wore — sloppy clothes. Anyhow, you said you spent six years with Krebs at Oxford...
- HK: What happened was that I spent six years from 1955, when I came back until 1961, when I physically moved to Leicester, with Krebs in Oxford although working very much independently after the first year.
- VM: And you went to Leicester for a Chair in Biochemistry?
- HK: Yes. That was my first academic appointment.
- VM: How long did you stay there?
- HK: Fifteen years.
- VM: So that took you to 1975.
- HK: 1975, yes.
- VM: And then you went to Cambridge.
- HK: I was appointed in '60 but I moved in '71 and stayed there for 15 years building up the school and then became Sir William Dunn Professor at Cambridge from '75 'til '95.
- VM: And here you are now with a new life and renewed living in Boston.
- HK: "Recycled" is the word you're looking for.
- VM: What's your formal title here?
- HK: At the moment the Acting Director of the University Professor's Program. I am a University Professor and a Professor of Biology.
- VM: I see.
- HK: Very important.
- VM: Well, thank you very much. I think that in spite of your having not remembered anything we have a number of useful reminiscences and views and thank you, indeed.
- HK: Well, at this point the only name with which you can charge me, the only name I can think of is Alzheimer, but thank you.

VM: Well, most of the chaps we deal with are fairly elderly and you are less Alzheimic than many of them, that's for sure! OK. Thank you.

This form has been completed on the basis of information provided
by Professor Kornberg.

Y
16 26/7/96

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name HANS LEO KORNBERG

Date of birth 14 JANUARY 1928 Birthplace GERMANY

Father's full name MAX KORNBERG

Occupation MERCHANT Birthplace NR. BRUNSWICK, GERMANY

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Occupation _____ Birthplace NR. LIPPE, GERMANY.

Your spouse DONNA KORNBERG

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Your children 2 SONS (RESIDENT IN HONG KONG AND SINGAPORE)

2 DAUGHTERS (RESIDENT IN ENGLAND)

Where did you grow up? NR HERFORD GERMANY UNTIL 10, THEN ENGLAND

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Education VARIOUS BOARDING SCHOOLS

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Occupation(s) PROFESSOR OF BIOCHEMISTRY - NOW UNIV. PROFESSOR

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Areas of expertise BIOCHEMISTRY, ESPECIALLY MOLECULAR BASIS

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Other interests or activities COOKING, CONVERSATION AND SAILING

Organizations in which you are active ROYAL SOCIETY, NATIONAL ACADEMY
OF SCIENCES, OTHER ACADEMIES, BIOCHEMICAL SOCIETY, ETC.

Chapter 34

WILLIAM STEPKA

Richmond, Virginia

July 28th, 1996

VM = Vivian Moses; WS = William Stepka; SM = Sheila Moses

VM: This is a conversation with Bill Stepka in Richmond, Virginia on July 28th, 1996.

Bill, you had a rather complex path that led you to Calvin involving your early scientific career. Why don't you tell us about it?

WS: Well, the story starts when I was separated from the Air Force and returned to the University of Rochester where I had been doing undergraduate work before I went into the Air Force. I served in the Air Force for a little over three years. At the University of Rochester I ran into an Englishman who was in a laboratory with Bob Dout (*spelling?*) in London and he came back to visit this country and was being groomed for the chairmanship of the Biology Department at the University of Rochester. His name was F.C. Steward and he was interested in some amino acid metabolism in potato slices and we were going to do research on the metabolism of the potato slices as they healed. So, since I had been out of the academic environment for over three years, I had a lot of library research to do and so I spent a good bit of time in the library looking over the journals. I came across an article by Gordon (*Editor: this should be "Consdén"*), Martin and Synge describing a new method of analysis called "paper chromatography". That really struck my fancy because it was a method that seemed very easy and simple to me in contrast to the very difficult analytical schemes that the analytical chemists had prescribed prior to that.

VM: Can I ask you a question — what was your own scientific background? Were you yourself a chemist?

WS: No. I was a major in biology in my pre-war years.

VM: You had been at Rochester before you went into the Air Force?

WS: Yes.

VM: How long had you spent there?

WS: Actually, I joined the reserves in my junior year with the promise that juniors and seniors who joined the reserves would be permitted to finish their degree. But something happened and I got called a month early. At the beginning of my senior year at the University of Rochester, they passed some sort of a resolution recognising the fact that some of the students might be leaving so they decreed that any student, any senior, who was in good standing on February 20th or some such date in February would automatically would get his degree. I got called one month earlier. All of my professors and the powers at the University of Rochester said "no, this is a mistake, just keep on going to classes. Pay no attention; we'll get this straightened out". But it didn't happen so I had to leave the University on the 20th of January and proceeded to Boca Raton, Florida, for basic training for the Army Air Force Technical Training Command.

SM: What year was that?

WS: That was in January of 1943.

VM: So you didn't get your degree at that time?

WS: No, I had to finish a semester.

VM: And that was the immediate reason for going back to Rochester to finish off?

WS: That's right. And I got back to Rochester on February 6th, 1946.

VM: And so in what capacity did you meet Steward?

WS: Well, I had been a Teaching Assistant before I left for the service and I guess they needed Teaching Assistants. It was actually David R. Goddard, who was the chairman who had hired Steward to come to Rochester, with whom I was getting my degree and who hired me as a Teaching Assistant for the balance of the semester.

VM: Sorry, I left you in the library looking up...

WS: So we got to paper chromatography, the article by Gordon (*Consden*), Martin and Synge, and then I immediately set up some equipment to test this method and, indeed, ...this was just unidimensional because what I used was a large, inverted bell jar with a microscope dish (a slide dish) for holding the paper and the solvent. Indeed, we found amino acids from the potato slices using phenol as the solvent. Then we got some collidine, which was the solvent described by Gordon (*Consden*), Martin and Synge and that didn't work. There was no separation; it was absolute disaster. Then one day — at the Medical School...at the Medical School every week there was sort of a list of seminars which was published — and I saw a seminar listed at the Medical School by a Charles E. Dent and the title of his talk was "Paper Chromatography as a Diagnostic Tool in Rare Liver and Kidney Diseases".

In the meantime the only other person I knew who had tried paper chromatography was a friend of mine, Dr. Patton at Cornell University who was an insect physiologist and he worked on...he was analysing insect blood and therefore microtechniques were of great value to him. So he tried. He also...we had been friends before I went into the service and we were in contact and we discussed our experiences with paper chromatography and both of us agreed that phenol worked fine but collidine didn't. So we were unable to get 2-dimensional chromatograms as described by Gordon (*Consden*), Martin and Synge. So I called him and invited him to come to hear Dent

at the seminar. He and his students started out from Ithaca but this was in April and they got stuck in a snow storm and didn't get there until the seminar was over. Dent had these beautiful chromatograms showing nice purple spots with amino acids on the sheet so after the seminar I went up and I talked to him and told him about my experiences so he said, "Oh, you must be doing something wrong." I said, "Yes, but what is it?" He gave me some advice and I went back and I tried whatever he suggested and this went back and forth several times none of the stuff worked. And one day I was talking to him in his office and we were discussing the possibilities and then he said, "Would you mind coming with me? I have some chromatograms to take out of the cabinet." He had sort of an inside room for his chromatography room where the temperature was a little more stable. And I followed him and he was saving his — he had a large separatory funnel and he was saving the residue of the solvent in the troughs and poured it back into the separatory funnel. I looked at this and I noticed that his collidine was coloured, it was quite yellowish, and mine was clear. I mentioned this to him so he said, "bring me some of your collidine." So I brought him some of the collidine and he tested it on samples he had been getting good results with the collidine he brought with him from England and that's why he was saving it. He didn't use any American collidine. So I gave him some of the collidine that we had and he tried it and he got the same results that I did — no separation.

When we discussed that, we decided that his collidine he brought from England must be contaminated with something and that I had a purer sample than he did. The only source of collidine at that time in the United States was Riley Tar Chemical Corporation — I think they were in St. Louis — and so we wrote to them and asked them what possible contaminants there might be in a fraction called collidine and they wrote back and said there were several suggestions...and could we have samples. They wrote back and said the possibilities were quinolines and lutidines and they sent about eight samples, I think. When we lined up the samples that we got, we immediately rejected the quinolines because they were too orange. The lutidines looked about right.

VM: In yellow colour?

WS: Yes, the yellow colour. So we took the collidine that I had and kept on adding lutidine and immediately the results improved as we added but we still weren't getting the same R_f values that he was getting with his British collidine. So it wasn't until we had 50% lutidine and 50% collidine that we were able to reproduce the map of amino acids that he had.

VM: This was using phenol in one direction and collidine/lutidine in the other?

WS: Yes. The disadvantage of the collidine and lutidine mixtures is that they have a very offensive smell. They are almost like pyridine. So we were using and doing some work in Steward's laboratory with these things and I immediately thought at the time, "gee what a wonderful tool this would be for photosynthesis" because C^{14} , you see, had recently been discovered and that there would be a great possibility of using this and using radioautography along with it.

VM: Can I ask you some questions now? What did you know about the state of photosynthesis research at that time?

WS: Very little. I didn't even know much about Calvin's work because I hadn't caught up with that literature. I didn't learn about Calvin's work until I got to Berkeley.

VM: You didn't even know about it?

WS: No. This was in '46.

VM: This was a thought you had quite independently of Calvin about how you could use it?

WS: Oh, yes, yes.

VM: And the second question I have: was radioautography a technique which was in use at that time?

WS: Chargaff had used some phosphorus-labelled compounds. He was looking at phosphorus-labelled compounds and unidimensional photographs. There was a paper published in *Science* but the pictures of the chromatograms didn't look very good, to me at any rate...separations...But the idea that you could use radioautography and expose the chromatogram to a film was certainly there.

VM: And the last question I have to ask is what sort of paper did you use at that time because presumably...?

WS: Whatman No. 1.

VM: Was that available in large sheets?

WS: Yes, it was, by special order. It was rather expensive at the time; I forget how much we paid for a sheet.

VM: Because if that was before general use of chromatography. I wonder why Whatman produced large sheets of the paper? What they did it for.

WS: Well, Gordon (*Consden*), Martin and Synge already had used the Whatman No. 1 paper.

VM: Oh, had they?

WS: Yes. That was prescribed in the original article.

VM: I see. So it pre-dated all of chromatography. It had been made for some other purpose.

WS: I don't know why they manufactured it.

VM: Well, it might be interesting to talk to Whatman to find out. OK.

WS: To find what they were doing.

VM: Sorry. I interrupted you when you had just realised the value of this for photosynthesis.

WS: The offensive odour of the collidine/lutidine mixtures made me look into some other types of solvents and I noticed that the solvents that had been published in the literature — by now people were chromatographing sugars, amino acids and other things and there were some other solvents prescribed — and there was one that was based on butanol and acetic acid. But as published, it didn't provide very good

separations of amino acids and it looked as if the proportion of water was wrong. So I set up a phase diagram of butanol, acetic acid and water and did the phase diagram and I chose a spot of the three components on the diagram which provided about 12% water. I don't remember the exact formula at the moment. But anyway, that was a substitute for the collidine and gave R_f values very similar to collidine and lutidine except for the basic amino acids which, of course, were affected by the acetic acid in the components. So that was a better solution.

Then while I was there at the University of Rochester, a professor from the University of California came for a visit. He had known F.C. Steward in his student days in Hoagland's laboratory and he was on a sabbatical and was sort of travelling around the country and he came to the University of Rochester. His name was Professor Roy Overstreet. He lived in a dormitory on campus and we used to have dinner together and then in the evenings he was also a connoisseur of wines and we used to test the state wines in my office and lab. He was there maybe two or three months. At the time that he was leaving he said, "You know, Bill, I've gotten to know you pretty well. There may come a time when you and F.C. Steward are not going to get along and if you have any kind of trouble, call me. I think I'll have a place for you at the University of California." And, indeed, I thought that if I stayed much longer I might be mowing the professor's lawn eventually. So I thought that the best thing might be to leave so at the end of the semester I decided to go to California and wrote him. Indeed, they had a fellowship for me and I went to Hilgard Hall.

VM: As a graduate student?

WS: As a graduate student in Berkeley.

SM: Which year was that?

WS: That was in 1947, I guess, fall of 1947. I had already ordered solvents and things for the lab. where I was going to be working. I was working with Professor Lewis Jacobson and Dr. Roy Overstreet who were both interested in ion uptake by plant roots. We were actually working on the ion uptake by plant roots and their effect on the metabolism of the roots and I was using paper chromatography to see if there were any changes in the component extracts, in the extracts. While we were doing this I kept egging both of them, Jacobson and Overstreet, to see if they could get some carbon-14 as we had an ideal situation here for analysing the early components of photosynthesis, metabolites of photosynthesis. Eventually Dr. Overstreet, who frequently ate lunch at the Faculty Club, spoke to Dr. Calvin.

I have to backtrack a little bit. In the meantime I had seen, I had heard two seminars, one given by Calvin and one given by Andy Benson, about photosynthesis and in the question period after the seminars I inquired about using paper chromatography and they had no inkling of paper chromatography as a method of separation. So Professor Overstreet eventually spoke to Dr. Calvin at the Faculty Club and arranged a meeting. So I brought along some chromatograms that we had. By then I knew the positions; I had a map of amino acids, sugars and some of the phosphorylated compounds. I brought this along and Professor Calvin was very impressed with this technique and he wanted me to take some samples that they had prepared and had been analysing and see what might happen if I chromatographed the samples. So I took them back to Hilgard Hall, took the samples, and I had great difficulty in applying the sample to the origin of the chromatogram. It looked as if the paper was waterproofed by the sample.

VM: What sort of samples were these that they gave you?

WS: These were samples that they had prepared. They were samples of *Chlorella* that had been exposed to carbon-14 in the lollipop for one minute and then they fractionated them and gave me a nice lot of clear...

VM: They fractionated them on ion exchange?

WS: Yes. I don't know what was the entire technique that they used but they had gone through ion exchange resins. At that time Dr. Calvin was a consultant to Dow Chemical Corporation and they were just developing ion exchange resins so he brought back samples and they were using them. I guess in the early days there were different degrees of cross linkages in the resins and I guess they used some of the resins that were of the lower cross linkages so that they were somewhat soluble and starting with the initial fraction because what they did when they exposed the *Chlorella* cells to carbon-14 to stop the metabolism quickly, they dumped the exposed *Chlorella* into what they called the "hemlock mixture" which was a concoction of trichloroacetic acid, hydrochloric acid and alcohol, boiling, so there was a lot of fractionation to do...see, they had to get rid of all the trichloroacetic acid and the hydrochloric acid. The result was...in trying to apply the sample to the origin the result was that...because I had to guess how much I should put on because I had no idea what volume of cells, *Chlorella* cells this represented at the end and it was sort of by-guess-and-by-gosh but I tried to get on as much as I could.

The separations were not satisfactory at all because apparently this waterproofing material held...Most of the radioactivity stayed at the origin and a little bit of it smeared off in both directions but not much. So after several such attempts without any success, I decided there wasn't any point in running any more of these samples. We were discussing these results with Dr. Calvin and I said, "Why don't you use the simpler methods? Why don't you just kill the *Chlorella* in hot alcohol, which is easy to get rid of, and then concentrate the material and give it to me as is and let the paper do the separation instead of ion exchange resins and other chemical procedures?" So he apparently thought that was a good idea so he said, "Why don't you tell Andy how to do the experiment and then do it your way." So we did. About four o'clock in the afternoon we started the experiment and did what was called a "1-second exposure"...just virtually dumped a lollipop into hot alcohol quickly and then concentrated it very quickly and I washed the green gunk out of the *Chlorella* with alcohol, with maybe 25% alcohol or so, and took it back and ran some chromatograms, made some radioautographs a week later, looked at the radioautographs and lo and behold I counted 30-some compounds. Many of them I could identify — all the amino acids, of course, that were there I could identify right away. I should say also the organic acids I had already mapped and some of the phosphates, sugars — and brought the results to Calvin and he was absolutely amazed at what he saw. As a matter of fact, he was scheduled to give a Sigma Xi lecture that evening and so he kept the chromatogram and had a slide made. There were no labels on the spots at the time and, indeed, in the evening, when we went to hear him, he showed the slide and he said something to the effect "We have been working for 3-4 years", I think he said, "and we had discovered radioactivity". I think the only compounds they had identified until then was malic acid, which was indestructible in the "hemlock mixture", you see. And then he said, "And then we discovered paper chromatography," and showed the slide.

VM: Did he acknowledge how the discovery was made?

- WS: No, no. "Then we discovered paper chromatography" and he was pointing to spots and I was sitting in the audience and he was asking me to identify the spots as he was pointing with his pointer. In the back of the room Michael Doudoroff had been there and he had received his copy of the *Journal of Biochemistry*, I think it was, where Loewer (*spelling?*) and Gardini had discovered glucose-1-6-diphosphate. It was that week that this publication came out. We didn't have our (*copy*) because that was being catalogued in the library so none of us knew about glucose-1-6-diphosphate. Doudoroff asked, "Is there any evidence of glucose-1-6-diphosphate in the...?" and Calvin took his pointer and pointed and said, "Well, we think it might be this one".
- VM: But he made that up on the spot, did he? (*Laughter*)
- WS: I guess so. So I turned to John Weigl who was sitting next to me and I said, "We have our work cut out for us for the next two years to catch up with the seminar".
- VM: But you weren't formally an affiliate of Calvin's group at that time, were you?
- WS: Not at that time, not yet. See, all the chromatography was still being done in plant nutrition, Dr. Jacobson's laboratory.
- VM: Was he your thesis supervisor or research director at the time?
- WS: No. Not at the time.
- VM: Who was?
- WS: All of my thesis committee was composed of people from Plant Nutrition and Biochemistry, all people from Hilgard Hall and the Life Science Building.
- VM: And you were notionally working on a problem of plant nutrition?
- WS: That's right, yes. I also identified...confirmed the presence of sucrose not only by co-chromatography but I took some invertase...I eluted a spot, a radioactive spot, treated with some invertase solution and then ran a chromatogram and, indeed, I got glucose and fructose. Now, the interesting thing was that when the radioactivity was compared between the two, one of them had more (*radio*)activity than the other (I don't remember which was which) which was, of course, some indication as to what the pathway might be. It was at that point, I think...well, quickly Calvin had cabinets made and a chromatography room set up. I think it was Al Bassham who first utilised paper chromatography to a great extent and he did a great job at analysing because he did the same thing with the sucrose and separated it and then actually broke it up atom by atom and was able to quantitate the radioactivity in each one of the six carbons for each sugar. That was a very great...By then, let's see, I had an AEC Pre-doctoral Fellowship and I guess Calvin invited me to join the lab. and that's when I moved over to the Radiation Lab. and worked at the Radiation Lab.
- VM: Sam Aronoff remembered you coming and telling him and Vicky Lynch about paper chromatography.
- WS: Yes, yes, yes.
- VM: And that was before you told Calvin or afterwards do you think?

- WS: No, yes, that was before, yes, because I was trying to...I was always trying to get my hands on some C^{14} and I thought that maybe through them, since they had access to C^{14} and I explained what I would like to use it for. Yes; that was true.
- VM: So then when you came into that lab. you worked in the Old Radiation Lab., the old wooden building?
- WS: Yes.
- VM: In the big room?
- WS: In the big room right on the spot where the floor had been replaced because of radioactivity where the old cyclotron used to sit.
- VM: Who was there, who was in the building when you went in?
- WS: Let's see. Al Bassham worked on the other side of the bench from where I was working. Dick Lemmon was there, Murray Goodman and, of course, Vicky Haas Lynch (she was then still Vicky Haas). Let me see who else. There was some medical student who went to Harvard; his name was Gordo. He worked there in the summers, that I remember. There was an engineer who was working full time designing equipment. He put together the first CO_2 analyser, built it from scratch. I don't remember his name. He went to work for Bechtel Corporation eventually.
- VM: Was John Weigl there?
- WS: Yes, John Weigl was also there.
- VM: Of the graduate students, you were one and I recognise that Murray Goodman was a graduate student. Was John Weigl a graduate student?
- WS: John Weigl was a graduate student at the time. I think Al, Al Bassham was still a graduate student. His thesis was on quantitating the radioactivity in the individual carbon atoms of the sugars. That was the substance of his thesis.
- VM: And I guess Andy was there?
- WS: Andy, of course, Andy Benson was there. He was sort of the major-domo as he was Calvin's right-hand man.
- VM: Did you see Calvin often in the building?
- WS: Oh, yes. I think he was in every day. Once a week we had a seminar reporting on what we had accomplished during the week and most of those results got published very quickly. The group was very intellectually stimulating. It was a Mecca for scientists from all over the world, actually. I met all sorts of people there: people like Linus Pauling used to come in for seminars, Glintz (*spelling?*) from Stanford. It was really intellectually a very stimulating group. I remember we used to eat in some student cafeteria some place and the discussions ranged from all sorts of topics. And there were also some of the physics people. I was sort of interested in physics because a graduate student of Lawrence's, E.O. Lawrence's, was E.O. Lawrence's first graduate student, and he was my classmate at the University of Rochester and he eventually succeeded Lawrence as Director of the Radiation Laboratory (*Editor: of*

the Lawrence Livermore National Laboratory) and was President Eisenhower's Scientific Advisor.

VM: What is his name?

WS: Herbert York. I guess he made several trips to Geneva negotiating with the Russians on nuclear affairs. He recently wrote a very interesting book called, *Making Weapons, Talking Peace* which is very interesting. It gives a nice history of the Radiation Lab. there. So I often used to attend the physics seminars up on the Hill at the Radiation Lab.

VM: And that have been the building in which Lawrence had his original 37" cyclotron, I think? The ORL, Old Radiation Lab.

WS: Yes, the Old Radiation Lab.

VM: Did Lawrence come in there at all?

WS: Yes, Lawrence had a laboratory up on the second floor of the Old Radiation Building and he used to come in at the time. He was working on a colour TV tube and was running some experiments. Apparently the tube worked but it wasn't commercially successful. I think he succeeded in developing a tube that showed colour. Yes, he was quite an interesting man, I thought.

VM: Did he come into Calvin's lab and talk to you people about what you were doing?

WS: Not very much. I think he got most of his information from Calvin himself. Occasionally he would come in from time to time. Usually he would come in when he had some visiting firemen and he would show them the whole operation and this was, of course, a part of his operation so he brought the visitors in from time to time.

VM: So he was regarded as a great man at the time, was he, and someone to be in awe of?

WS: Oh, yes. You know that group up on the hill...there was Seaborg there.

SM: Last night you described the atmosphere in the seminars and the arrival of E.O. Lawrence. Would you like to do it again?

WS: All right. Well, E.O. was a very revered man and the physics seminars which were held in a sort of an army type barracks up on the hill. People would assemble sort of before the appointed time — I think the appointed time was four o'clock in the afternoon — and everybody would be sitting down quietly and E.O. would stride in and walk down the centre of the aisle and he had a leather chair sitting up front and he would walk up to the front of the room, turn around and bow and sit down and then whoever was introducing the speaker would get up and introduce the speaker for the afternoon. The seminar could begin when E.O. was sitting in his leather chair. That was a weekly ritual.

VM: Did he interrupt the seminar speakers as they spoke?

WS: I don't recall that. I don't recall that he was a great participant in the discussion.

VM: But you recall that Calvin was a participant in the discussion?

- WS: Oh, yes. Calvin was a participant in any seminar and on any topic. You know he had a
- VM: Coming back to the development of chromatography, paper chromatography, in ORL you said that Calvin had cabinets made and so on. Did you participate in the design?
- WS: Yes, I designed them, yes.
- VM: And you designed the troughs?
- WS: Yes, they were made by the glassblower next door. I don't know if you remember that there was a glassblowing shop right next door. I don't remember the glassblower's name anymore — was it Mr. Powell?
- VM: I don't remember.
- WS: I don't remember his name. But he devised a way of making the troughs. He took a glass tube of the proper diameter (I don't remember what diameter that was), sealed both ends and flattened the ends. And he had a diamond saw and he ran the diamond saw and made two troughs in one pass.
- VM: And these were long ones.
- WS: Yes, enough to take the 24" ...they were about 26". There was an inch to spare on each side after the long dimension of the paper was immersed in the trough.
- VM: All of this happened before my time. But when I got there they were using stainless steel and it was rather different. That was a later design. But in the early days when you had...
- WS: I think maybe we had some stainless steel made while I was still there.
- VM: Right. And ones which had a sleeve over a rod and you clipped the paper to the sleeve so you could take the whole thing out to dry.
- WS: Right, right. They had little indentations...the ends had little indentations for the rods over which the paper passed. I think I may have participated in the design of those. I had written a chapter for *Methods in Medical Research* on paper chromatography. I was asked to do that by Professor Cho Hao Li, do you remember him in Biochemistry, the chromosome man? That was while I was still...that was my first year there when he learned I was doing paper chromatography. He wanted me to...he was co-editor, I think it was for Volume II of *Methods in Medical Research* and he had asked me to write a chapter on paper chromatography for that volume.
- VM: Do you still have a copy?
- WS: No, that copy doesn't exist but that's not the end of the story. I was very reluctant to do this but he eventually talked me into it. He wanted the article to cover amino acids and proteins and I felt very uncomfortable about the protein part so he said, "Well, I'll ask my graduate student, George Hess, to do the protein part if you do the amino acid part". So I did the amino acid part and George didn't get around to doing the protein part so eventually I wound up doing the protein part also. Then Li went off to Sweden for a sabbatical. I remember ...the editor of that volume was Gerard from the University of Chicago and I remember getting a frantic call from Gerard wanting to know where this manuscript was. He had sent it back for condensation; it was too

long in the original form and he had sent it to Li for condensation. And I said I had no idea ...I didn't have a copy of this and Li was in Sweden. And he said, "Go see if you can find it on his desk somewhere". So I went to Li's secretary and she found this thing.

So I cancelled a Christmas vacation trip down to Pebble Beach, I think it was, and I was editing and trying to shorten this article. In the meantime, apparently, another copy got to Li in Sweden, who was doing the same thing. But Li, of course, having no experience at all with paper chromatography, used scissors and glue type of editing. That was the copy that was actually returned to Gerard and that was printed and I got a page proof copy of this. I looked at it and I was absolutely horrified to see what had been done to the paper and I didn't want it published. Even the photographs were upside down — they didn't know enough to know which way the photographs went.

Tape turned over

So I insisted that the chapter be pulled from the book and eventually that is what happened and the paper was not published. Subsequently, in two or three years, under the editorship of Corker and for Volume V of *Methods in Medical Research* Lyman Craig invited me to brush off the old paper and add some new information and submit it again so I did that and the paper was published in Volume V of *Methods in Medical Research*.

VM: Were you actually a graduate student when you wrote the paper?

WS: Yes, yes.

VM: Did Calvin know about it, did Calvin know you were doing it?

WS: I don't think so because I started it before I joined Calvin's laboratory. The paper was almost finished before I joined Calvin's laboratory

VM: And it was just published under your name?

WS: It was submitted under my name, yes. But when I got it back in this horrible page proof, Li had added his name as co-author. I guess he felt he was entitled to do this because of the editing he had done!

VM: Can we talk a bit about the research work that you did for own thesis. You spent a long time developing techniques and methodologies and then you applied them to the problem?

WS: Yes. The question at that time, because Calvin's thesis was that photosynthesis was a reversal of the fermentation (the Embden-Meyerhoff cycle) and driven by the energy from light and that carbon dioxide passed through all these intermediates on its way to sugar. There was a body of evidence in the literature which showed that green plants, although they have the capacity for fermentation reactions, often lost that capacity through time. Mostly the tissues that were capable of fermentations were the thick tissues like the cotyledons of peas and others and as the seedlings developed the leaves, for instance, would lose the capacity for fermentation which, of course, made me suspicious about the theory of a reversal of the fermentation process.

So I set out to design experiments to test this theory. One of the tools that was used by the early people who worked with muscle and yeast fermentation was to poison

enzymes and in that way find out something about the nature of the process. And one of the enzymes that was, of course, in the fermentative course of reactions was triosephosphate dehydrogenase which is susceptible to inhibition by iodoacetamide. So it occurred to me that the reason that if carbon dioxide had to pass from phosphoglyceric acid through triosephosphate dehydrogenase through the triosephosphate *en route* to the hexose sugars that, if the cells were poisoned by iodoacetamide, that the phosphoglyceric acid should accumulate and the sugars should decrease. An experiment was designed to do this and the results showed that phosphoglyceric acid did not, indeed, accumulate but the amount of sucrose increased with the increase in time of exposure of the cells to iodoacetamide prior to feeding the carbon dioxide. So that, of course, cast doubt on the possibilities so I checked the possibility in another way. This was going from carbon dioxide to sugar. Then I tested the cells' ability to convert sugar to carbon dioxide and water again, feeding them labelled sucrose and labelled glucose.

VM: In a respiratory sense?

WS: Yes. I did it aerobically and anaerobically and compared the results and again used iodoacetamide to inhibit the triosephosphate dehydrogenase. In this case we would have expected sugar...yes, an increase in the triosephosphates on the chromatograms and a decrease in the phosphoglyceric acid and again we didn't obtain this result; we obtained the opposite result. So the conclusion was, in the thesis, that photosynthesis cannot be a reversal of an unmodified path reversal of the glycolytic sequence of reactions. The thesis was submitted and that was in line with some of the results by an Egyptian student in Biochemistry who was Paul Stumpf's graduate student, who tested a large number of plants for triosephosphate dehydrogenase and found that, indeed, some of the plants lacked this capability of doing this.

VM: Did you specifically test whether the triosephosphate dehydrogenase was susceptible to iodoacetamide?

WS: Well, that had been established...

VM: In the plant enzymes?

WS: Yes. That had been well established in the literature. And I used the same concentrations that were suggested in the literature. There was a good value of evidence that triosephosphate dehydrogenase, indeed, is inhibited by iodoacetamide in plants.

VM: Who was on your thesis committee?

WS: Roy Overstreet was the chairman, Paul Stumpf was on my thesis committee...let's see, who else?...A.C. Krafts, and I guess Calvin was on my committee also.

VM: How did he react to your conclusions?

WS: He accepted them but he thought that iodoacetamide might do other things besides inhibiting triosephosphate dehydrogenase. As a matter of fact, when he saw the great increase, the 12-fold increase, in sucrose after treatment with iodoacetamide he actually persuaded a sugar beet company (in Idaho, I think it was) to spray the sugar beets with iodoacetamide to increase the sugar concentration. Obviously this was a failure because of the exposure with time, the other reactions and the uptake of CO₂ eventually decreases and declines anyway.

- VM: With hindsight how would you now interpret what you found with the subsequent understanding of the system?
- WS: Well, I really don't know. I haven't discussed this problem with anybody for years. At the time there were many people — Martin Gibbs — who thought maybe that there was something to this and maybe it deserved further investigation. As a matter of fact, I sent Martin Gibbs a copy of my thesis as he asked to see it. I think maybe he had done some experiments but I have never heard the results.
- VM: So you submitted your thesis in about '52 did you?
- WS: Yes, I think about '52.
- VM: During your stay in Berkeley you'd been married, hadn't you?
- WS: I got married in...on July 15, 1948 to a wonderful lady, Bonnie Jean Thomas, whom I met as a graduate student at the University of Rochester when I returned from the service.
- VM: And she worked in Berkeley too?
- WS: She worked in Donner Lab. She worked for Dr. Tobias and irradiated thousands of mice with the small cyclotron next door to the Old Radiation Laboratory.
- VM: That was the one in Crocker?
- WS: Yes.
- VM: So you presumably, both through her and through your ORL connection, knew the Calvin Donner people as well?
- WS: Yes.
- VM: Was there, in your experience, a lot of mixing of people between the two locations in Calvin's group or were they rather separate?
- WS: Yes, once a week we used to meet — Calvin's group, that is the people who were at ORL proper used to hold a seminar in one of the rooms in Donner and many of the people in Donner used to come to our seminars so there was a good deal of interaction.
- VM: What were the social relationships like among the people in the group? Did you spend a lot of your spare time together?
- WS: Well, yes, there were a lot of...most of the people used to spend a lot of time going up to the mountains on weekends although, unfortunately, I used the weekends for work because it was the only time that the counters down in the basement were available and free so there was no competition for the counters so I used my weekends generally and I didn't participate in these excursions to Big Meadow and various other places that they liked to go. But Dick Lemmon, Al Bassham and Andy Benson all liked to go. But we used to have lunch together and there were often...in some cafeteria down on campus, not very far. I don't remember exactly where it was but I seem to recall that it was near a girls' hockey field somewhere, sort of towards town

from the ORL and past the Faculty Club somewhere, and those were very fine and stimulating luncheons.

VM: So there was a lot of technical discussions within the group between people?

WS: Oh, yes.

VM: Was the big white table there when you were there on which people would lay out chromatograms? Many people have spoken of it and, indeed, the table still exists. It is a large table, probably bigger than your dining table, with a white Formica top and drawers underneath. People used to stand around and lay these chromatograms out. There was enough space to do this.

WS: I don't think so. I used to use some table in the chromatography room upstairs, mostly, I think, to examine my chromatograms.

VM: So what was the social focus in the building like? What did people do at coffee time? Did they gather together at coffee time?

WS: I don't remember spending a great deal of time with coffee. If we had coffee, I think we just took it to our benches and drank it there.

VM: Many people to whom we have spoken have been very fond, very impressed with the structure of ORL as a building for facilitating interaction between people because it was relatively open and...

WS: Yes, that's right. You could talk to anybody in the room that you wanted to, for instance. I remember...I think there were what?...three or four of benches...

VM: Yes, something like that.

WS: ...with a desk at each end and they were sort of islands. There was a graduate student working at each side of the...

VM: With a chemical rack behind you.

WS: Yes, yes.

VM: So you were always chatting with...

WS: Right. There was always somebody just across the table from you, the bench from you with whom you could converse. And I happened to have the good fortune of being across from Al Bassham.

VM: And then there was this underground counting room which was already there when you were there?

WS: Oh yes. It was some three feet, six feet of concrete. It was shielded by six feet of concrete, I guess. But even so, on some weekends when the cyclotron had full power, the background was about 200-500 counts per minute.

VM: That counting room was designed as I...was used, anyway, specifically to count chromatograms and so as you were the one who introduced the group to paper chromatography they must have built it while you were in Berkeley whether you were

a member of the group then I don't know. But when you joined the group was it already there?

WS: Yes, it was already there. But there weren't that many counters there while I was there. Maybe there were two or at the most three counters and then eventually, I think, we had eight or nine counters.

VM: Was there a considerable time lapse between your first suggesting chromatography to them and the time you actually joined them? Because you talked about the things you did in Hilgard.

WS: Yes, I would say that it was almost...maybe almost a year before I actually joined the laboratory.

VM: And it was during that time, presumably, that they were building their own facilities.

WS: They didn't build their own facilities until I actually joined the laboratory because I designed the cabinets and the troughs and the equipment.

VM: I see. So the counting room in the early days was used for counting not paper samples, but was used for counting planchette samples and things of that sort. The counting of the papers presumably came with your own development of the chromatography.

WS: That's right, yes.

VM: And you fitted in, then, in that underground room.

WS: Right.

VM: And the chromatography room was actually built on the second floor, wasn't it?

WS: Yes. You had to go upstairs right next door to E.O. Lawrence's private little laboratory there.

VM: Oh, he still had a lab. in the building?

WS: Yes, he still had a lab. in the building.

VM: Did you have contact with him and did he with you?

WS: Yes. We had some contact but not a great deal. E.O. was not the kind of a person you talked socially with. He was strictly business and always in a hurry.

VM: When you left Berkeley, briefly what happened to you after then, between then and now?

WS: Well, let's see: we already talked about my thesis. After I finished my thesis, I had an offer to join the faculty at the University of Pennsylvania in Philadelphia so I left Berkeley and my wife and I drove across country, she doing the navigating mostly and we had a splendid time driving across the country. We did it slowly and stopped to visit my folks in Minnesota, which was the first time they had met Bonnie, my wife, and then we went on to Philadelphia.

SM: Which year was that? '52?

WS: I think it was '51, 1951.

VM: So you took a faculty job in Philadelphia?

WS: Yes. Let's see, I was an Assistant Professor of Botany at the University of Pennsylvania. And I stayed there four years. There I worked on sulphur metabolism. I had a graduate student who worked out the details of the reduction of sulphur. The plants take up sulphate ion and then obviously you don't find any sulphate compounds in plants. They are all reduced sulphur, sulphydryl groups. So he worked out the pathway on that.

VM: Was that using the same sort of technique you used...

WS: Yes. We used pretty much the same technique. Also we used some *in vitro* experiments. We grew *Chlorella* cells in sulphur-deficient media and then we tried to see what supplements would enable the cells to grow. Of course, they were unable to grow in the sulphate-deficient media and we were trying to see which intermediates we could...

VM: Which forms of sulphur would satisfy them?

WS: Yes. Let's see. I was at the University of Pennsylvania until 1955 and had an offer, was invited to come to the Medical College of Virginia to run a laboratory that had been donated by the American Tobacco Company. The Laboratory was called the Radiological Nutriculture Laboratory. It was a facility for growing plants in an atmosphere of radioactive carbon dioxide, in other words using plants as chemists to synthesise compounds which were difficult for organic chemists to synthesise. And also the compounds were uniformly labelled so that when they were fed to animals you didn't lose track of the...you could see all the pieces and all the metabolites.

We grew many crops of tobacco, obviously, and the idea was to see what happened to specific compounds when, for instance, a cigarette was spiked with a specific radioactive compound to see what happened to the compound as you smoked the cigarette, artificially, of course. We grew some flavouring agents. One of the flavouring agents used in tobacco comes from a plant called *Liaetris* which has a sort of vanilla-like flavour. So we grew that and we grew squill which was of some interest to farmers at the University of Kentucky and many other compounds.

It was an interesting place because it was a novel concept. It was just the second test facility, I guess, in the world. The Argonne National Laboratory had the first one. This laboratory was sort of designed with the experience of the Argonne people in mind so it was considerably improved over the one at Argonne, particularly in the way of leakage. It is very difficult to build a sort of a miniature greenhouse which is completely sealed so that the radioactivity doesn't escape. But we worked out the technique. It is also quite...it's a challenge to design a small enclosure in which you can grow plants where you have to maintain constant temperature. Lots of things happen because the plants are constantly transpiring, picking up water from the nutrient solution. We grew them in nutrient solution, aerated nutrient solution, so the plants are constantly transpiring water and that has a tendency to condense on the interior surfaces so that reduces the amount of light that the plants get and all sorts of problems. And then you must also remember that after you feed a certain volume of carbon dioxide to the plants in the enclosure, after they use that up, they kick out an

equal volume of oxygen. But the next time you feed them carbon dioxide, and that's assimilated by the plants, you get another volume of oxygen so the pressure keeps on increasing inside. So we devised a way of starting with a reduced pressure to accommodate the increases of the oxygen volume inside the chamber.

We had many visitors who were interested in the facility. There was a group of Japanese who came to inspect the facilities because they were interested in building one and I provided the plans and designs and had suggestions for improvements, and similarly a group from Germany who came. I know that the Germans actually built the thing. I don't know if the Japanese ever followed up and built their facility. So there was a facility at some institute in Germany whose name I no longer remember.

VM: Did any of the Berkeley people ever come and visit you?

WS: No, I don't recall any, no.

VM: Have you maintained contact with them?

WS: Not very much. I saw some of them at meetings in subsequent years but we didn't have any intimate contact.

The facility was used...After we isolated the compounds, you see, once we grew the plants now it was necessary to fractionate the thing and isolate specific compounds and we did this by chromatography a good bit and then it was possible to elute the radioactivity from chromatograms and feed the specific compounds to animals and also introduce specific compounds into cigarettes and smoke them and find out what happens to that. One of the things we...there was a laboratory in Sweden — Professor Schmitterlu (*spelling?*) in Sweden who devised a microtome which would take micron slices of whole rats, frozen rats, so what they did was feed some radioactive compounds to a rat, well there were several rats and then sacrifice rats at time intervals, sacrifice the rats in liquid nitrogen very quickly, and then sawed them in half right through the spinal cord and after that you could put the half on the microtome and take thin slices and take radioautographs of the thin slices and know exactly where the compound was, in which organs, and it turned out they used — we gave some of this nicotine to a Dr. McKinnes who was also a member of the Pharmacology Department of the Medical College of Virginia and he took it to Sweden to Schmitterlu's laboratory and they did that with the rats and it turns out that the nicotine very quickly goes into the brain and the liver. It is metabolised in the liver and also stays in the brain for a long time, as long as three weeks. One shot of nicotine to a mammal remains in the brain, traces of it, remain in the brain for a long period of time. Whether it is still nicotine or not that I can't tell. Some of the work indicated it was also...particularly in the liver because the liver could then be scraped out of the remaining...because you only used a thin slice of the rat. The rest of the liver was still there and you could take the liver out and analyse it and do other things with it. And then we also did...this was in the days when the Surgeon General declared cigarettes to be unhealthful and cancer causing...so we did an awful lot of work on the effects of smoke on animal tissues. One of the things we did was to study the effects of tobacco smoke on the cilia of cat trachea and it turns out that one puff of smoke sort of disorganises the nice coordinated activity of cilia and they become disorganised and after maybe ten puffs from some cigarettes the cilia reaction quits completely, stops. They recover but who knows what happens after repeated assaults over many years of smoking for instance?

VM: So all of this took you a fair way from photosynthesis...

WS: Yes.

VM: ... but you spent the rest of your career here in Richmond at the College of Virginia.

WS: Yes, I did. I retired in 1982.

VM: Looking back, and let's perhaps finish on this point, what did being in Calvin's group mean to you.

WS: I think it was very stimulating and very rewarding, particularly the people that I met there. And it was such a Mecca for visitors from all over the world. I met many famous scientists in Calvin's laboratory and benefited from discussions with Calvin's very active mind because he was...that was always an education.

VM: Well, I think we might stop there and like to thank you very much for your participation. It was a great pleasure coming to see you and it was very exciting being able to locate you because, as you know, the Berkeley people no longer had your address and so when I actually found you it wasn't too difficult, a couple of letters...It was nice to be able to come and see you.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name William Stepha
Date of birth April 13, 1917 Birthplace Veseli, Minnesota
Father's full name John James Stepha
Occupation Brick layer Birthplace Veseli, Minnesota
Mother's full name Mary Jasan Stepha
Occupation housewife Birthplace Montgomery, Minnesota
Your spouse Bonnie Gene Thomas
Occupation Researcher, Teacher, Housewife Birthplace Meadville, Pennsylvania
Your children Donald Thomas Stepha

Where did you grow up? Veseli, Minnesota

Present community Richmond, VA

Education BA University of Rochester, 1943

PhD University of California (Berkeley) 1951

Occupation(s) Professor Emeritus

Areas of expertise Plant Biochemistry; Pharmacognosy

Other interests or activities Gardening, Travel

Organizations in which you are active American Association of
University Professors (State & National)

Chapter 35

ROBERT RABSON

Rockville, Maryland

June 30th, 1996

VM = Vivian Moses; RR = Robert Rabson

VM: This is a conversation with Bob Rabson in Rockville, Maryland on July 30th, 1996.

As you know, I wanted to talk to you because of what you would have seen of Calvin's lab. from AEC Headquarters perspective, but before we talk about that, what was your own background because knowing what your own scientific training was will enable me to get a better idea of what you were looking at in his lab.

RR: OK. I got my Bachelor of Science degree and also my PhD degree at Cornell University working on plant physiology. While I was not working directly on photosynthesis, I was working on biochemistry of amino acids and various things like that. In any case, I did my postdoc., after I finished my PhD, with Ed Tolbert (Nathan Edward Tolbert) at Oak Ridge National Laboratory. There I did do some work on photosynthesis. As a matter of fact, we found some things that later on we understood better. For example, one of the things I was doing was checking radioactive labelled CO₂ and how it was incorporated into plants in a matter of seconds as well as minutes. One of the things we did with corn, was we found that there was malic acid that was one of the products. But at the time, the C₄ fixation was not yet understood. While we saw it, we didn't understand it. Later on people did. But we did a whole variety of work on photosynthetic systems. After I left the two-year postdoc., I got a job on the faculty at the University of Houston. I was the only plant science person in the whole department, as a matter of fact (*in*) the whole university. Nevertheless, I did go back to Oak Ridge for the summers, as well as going up to Beltsville, Maryland and working at USDA on different kinds of projects. They were not photosynthesis projects but they were plant science projects. And the during my fifth year at the University of Houston I was asked to come up to a Washington on a two-year rotation to serve the Atomic Energy Commission. Well, I was attracted to that and I decided, after getting permission, to go. Somewhat after the first year of serving in the Atomic Energy Commission, I was asked if I wouldn't stay on more permanently and I had to make a decision between going back to the university and staying on with AEC. I did enjoy the job with AEC. So, I made the decision not to go back and I informed the people at the university that I wouldn't come back. So I stayed on. That was in 1963 that I started the job at AEC.

During the next few years I had interactions with people in many different laboratories, both the national laboratories and university laboratories. Furthermore, in those days we did support projects outside the country, so I remember going down to Peru a number of times, Costa Rica a number of times, Brazil — and visiting the people who had the project and seeing what was being done, and so on and so forth.

VM: This was all in biological sciences, was it — as far as you were concerned?

RR: Yes. All those projects that I visited were projects having to do with either plants, in one or two cases entomology and so on but mostly plants.

VM: When did you first know Calvin? You had presumably known about his work when you were in the university.

RR: Definitely. I think it was maybe one or two years after I came (*to the AEC*) that I did pay a visit to Berkeley and meet him, that would have been in the mid-sixties sometime.

VM: What was your role in the AEC office?

RR: I was in the Office of Health and Environmental Research and my principal responsibilities had to do with plant science and, to some degree, with things like entomology. It was relating nuclear things with agricultural and other applications.

VM: Which had been part of the original mission of the AEC, wasn't it, to do that?

RR: As a matter of fact, that's one of the things that the objectives were in supporting the outfit out at Berkeley in photosynthesis. The whole idea was energy transformation and the use of radioisotopes to better understand (*these processes*). Anyway, we had some contacts with people in USDA who were using radioactivity to induce sterility and then control insects by releasing sterile insects into the wild and overwhelm the natural population. That turned out to be a quite interesting and effective way of controlling insects that were nuisance insects.

VM: Did you use a panel system for judging grant applications? What was the internal mechanism in your office for deciding who to fund, how much to fund them, and directing things in areas you thought of interest?

RR: Yes. In lots of the units of AEC and subsequent Energy Research and Development, and then the Department of Energy, lots of the units only used mail reviewers but as a unit...you see I had another unit that was started in 1979 and this was not in the Office of Health and Environmental Research (*OHER*): it was in Basic Energy Sciences. The project, the programme rather, was the only biological programme in basic energy sciences. You see, there was a tendency to want to introduce biological sciences into basic energy sciences. And so they went through a procedure and a decision was finally made that there would be a programme and then, after the programme was identified, they asked me if I would be the director. I said "OK". What they did was transfer something like four million dollars of the projects that I was working on in OHER over to Basic Energy Sciences. Ever since that time, the programme has been growing, so it's about 28 million dollars now. Also, at the beginning I only had myself and my secretary to handle things. We did use mail reviews and we established panels on an annual basis.

VM: So before then, in the early part of Calvin's lab, there had been no panels?

RR: Well, I wouldn't say there were no panels because there were quite a few instances of where people from the outside were called in to review a project. I remember that we would do that in taking the projects that we were supporting at Brookhaven and we would bring a number of people together who were experts in plant science and related things and have them listen to what they were doing, what they've accomplished, every three years. So that there was an outside review, people who were not associated with the Department of Energy/AEC, whatever. In any case, we did have — and do have — even though I have left, a system of using outside reviewers who were familiar with the technical aspects of the science to look at the projects, not only the ones that were going on but also the new things. During the panel meeting, once a year, we would review both renewal proposals and new proposals at the same time.

VM: In a unit like Calvin's, which had been supported by AEC for many years on an ongoing basis, there was, presumably, no likelihood that the funding would suddenly be terminated because the panel didn't like something. Nevertheless, you had to exercise proper control over where your funding was going. So how did you balance the AEC's appreciation of what the group was doing with what their ongoing research was, and the reputation: did you try to steer what they were doing? Did you comment back and say whatever, something you didn't like?

RR: If, by chance, they had come forth with a project that just seemed to repetitious and not particularly important, then we might raise a question. On the other hand, we did encourage that people do innovative things; that is, it's still being encouraged and it is, we feel, very important that with a programme that is not tremendously large that we do get involved in projects that are unique in many ways and innovative. It's not to say that every single project is that way but quite a few of them are. The other thing that we have done is run workshops where we would pick a topic that covered an area that was important but did not get enough attention...

VM: ...from the scientific community?

RR: Yes, from the scientific community. So we ran a workshop in, for example, plant biochemistry. This was some years ago, to bring out what needs to be done in plant biochemistry. We also ran a workshop on phytoremediation, that is, the use of plants to clean up things. There was so little work that was going on in that (*area*) and the work that was going on was not on trying to understand the basic functions of plants that relate to this. We tried to encourage people to start to do more basic science in order to understand what plants can do and how to manipulate, and so on and so forth.

VM: So Calvin's activities and those of his group in photosynthesis and what followed were very much the sort of thing you are interested in.

RR: Exactly. We, quite frankly, always sought people who really had great capabilities and a tendency to innovate in their research. We were delighted to have those people. The other thing that we recognised is that when you do innovative experiments, you may not get the results that are very meaningful right away because you would have to do many, many other experiments. So, in analysing the progress made, sometimes the people would not have tremendously convincing results but we appreciated the fact that in doing highly innovative work this may take time. We did not, at least in our group, cut back the budget because they didn't publish anything very exciting in the last few years. If they told us what they were doing and where they were going and it sounded exciting, we'd still support it.

VM: I have two questions at this point. One of the is: was this pattern of funding, and the attitudes you have described, influenced by the experience of the AEC in the Manhattan Project, the idea of big science and long-term developments and that you can't rush things?

RR: Well, I can't answer that in a very positive way, or negative way, because I just don't know. The point is that the AEC, back in those days, had lots of technical people in contrast to what's going on in government these days where the people who are in charge are oftentimes not technical people anymore. As a result, there's a certain lack of understanding as to how research proceeds, and this, that and the other thing. This is a little disappointing. Back in those days, in AEC days, the people were indeed quite technically oriented and they had objectives which were in many cases understanding basic scientific kinds of activities. They were not at all criticised for not doing something which was immediately related to radiation or something like that. One really fine example of that was Alex Hollander's laboratory down at Oak Ridge. He conducted — that is, the people in his lab. conducted some of the most outstanding genetics of that day and it sort of led into molecular biology. He really had the tendency to look for investigators who would do innovative things. I really felt that he was one of the people who really made enormous contributions to biology by the way he ran the lab., the kind of people he had in.

VM: OK; the second question was about the mechanism of Calvin's group in actually communicating with AEC office in funding terms. From the inmates point of view, we used to write up the year's work and project the future year's work, and so on. And I presume, I didn't see this, but I presume that this was accompanied by a budgetary statement of what their expenditure would be expected to be in the following year.

RR: That's correct.

VM: Then, what happened to that? Did you...was the relationship such that they knew pretty well what to ask for and what would be acceptable or did you have endless discussion about how much here and how much there? How did it go?

RR: This was highly variable because nobody knew, really early on, exactly what the budget would be. If it went through Congress, and sometimes the budget would be incremented and sometimes reduced, and we would encourage people to submit budgets of at least a prior year level and, if they came forth saying that we want to do this in the future and it's going to require this and that, if there was any hope that the budget would come through, we encouraged them to submit that expansion of the programme. Or otherwise, if we were convinced that this was an important thing to do and we knew new money isn't going to be coming, we suggested to them that maybe if they had something there which really wasn't that important they would transfer the money. These are complex things, and with each laboratory it's a different story.

VM: But it was a gentle relationship which relied on personal contact between your office and your colleagues and them...

RR: Yes.

VM: ...and you were in a position, both sides, to talk back and forth and work things out.

RR: Yeah. We definitely wanted to have these interchanges so we knew what they were doing and they would have an idea of what kind of resources we had available to go ahead.

VM: It's really running ahead of the period that we want to consider, because the detailed time that we are discussing, we're researching, is going to finish is 1963 which is the time when Calvin's group moved into that round building, you may remember.

RR: Right.

VM: We have to stop somewhere and that's a place to stop. To look forward, later in the sixties there was, of course, a gradual move away from complete AEC funding of Calvin's group and people then began to get grants from all over. I gather that is a procedure which has increased. Does that mean that AEC or, I guess, it's EPA, DOE, whoever the present body is, the present successor body...

RR: DOE.

VM: Is it DOE?

RR: Now.

VM: Yeah...does DOE now use the same approach as AEC did in your beginning days or has it also changed?

RR: It's changed considerably. One of the things that's changed, I have already mentioned. The people who are managing are not all familiar with technology although, I must say, that people in Basic Energy Sciences, for example, who run the different divisions are all technical types and do keep in touch with the people at the laboratories. But at the higher elevation, some of those people just don't understand what the importance of certain kinds of research is and they don't give it much in the way of the resources.

Let me give you an example. A number of years ago we held a workshop on carbohydrate structure and we had people from many different places and we had them from other agencies at the same time, about 25 people, but the whole idea (and there were some people from industry), the whole idea was to lay out what is needed in the way of carbohydrate structure procedures. One of the recommendations that the workshop people came up with was that there ought to be centres developed. Since I supported this, I went around afterwards showing people that this was an important thing and we didn't get a dollar to go ahead with this until there was another thing that came up and one of the Senators from a state wanted us to do something at one of his universities and wanted *us* to do it. He said that he would proceed to put the necessary money into it, in our budget. But one very good thing that he said, he said this would be only three years, and if that group is not competitive, no more funding.

VM: The atmosphere changed and it sounds as if it became politicised as well.

RR: Yeah. The thing that happened is that we got the money for that particular project but at the same time they gave us some extra money and that's what we used to start up the Complex Carbohydrate Center. What we did, we put an announcement in the *Federal Register*, asking if there are interested people to send in proposals. We got six proposals and we reviewed it with outsiders and the people from the outside felt that there were two excellent proposals and then we site-visited with the people and had

them give us additional opinions. Then we had to make a decision, either one or the other. We made the decision to pick it up at the University of Georgia. That particular project has worked extremely well.

It turned out later that the National Institutes of Health also contributed some money, not into the same budget that we had but an additional. They have gotten money from the outside, from different agencies, too. They have had literally dozens and dozens and dozens of people and institutions come in to work with them. They have provided means of gaining structural information about complex carbohydrates; they have provided courses for people to come and take for two, three, four weeks, people from industry to learn how to analyse things better and, of course, they have done research, too. I can show you a book that represents the responses of people to the Complex Carbohydrate Center and why they think it's important. In any case, we did not get recognition from the Department Of Energy, or anybody else, but we did find the money and I think it's recognised now.

VM: Have...I think, perhaps, this is the last thing I'd like to ask: have procedures become more bureaucratised than they were in the early days?

RR: Yes, quite indeed.

VM: Form-filling and things of that sort?

RR: Yeah. And the way the government is operated now, there is much more emphasis on the *way* the particular programme operates than what it achieves.

VM: I won't ask you to pass comment, but it's an intriguing thought to wonder how easy Calvin would have found it to get going had he started now compared to starting in 1945 when he did.

RR: Well, I think that there are agencies that do look for people doing innovative things that would produce information that indeed would have an impact. So I don't think that he would be impaired. He may not in today's time be able to start out with as much of a group that he had, but with time, it could build up.

VM: That's encouraging at least.

RR: I think this is very important. We have expressed that many times and other people who run programmes, both in DOE and in other agencies, have also expressed the importance of getting more innovative kind of things going. So, I'm not sure that every good scientist these days is being picked up because many of them are in the same area and there's already in many agencies lots and lots of money going into that particular area and they just can't...

VM: ...there has to be a limit.

RR: Yes.

VM: I want to thank you for spending time with us. It's a very valuable piece of illumination about the environment in which the group grew up and functioned. It's only by talking people like you who were at the other end of it that we can get that feel.

RR: It's my pleasure to meet with you and give you as much information as I can.

VM: Thank you very much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name ROBERT RABSON
Date of birth 4 MARCH 1926 Birthplace BROOKLYN, N.Y.
Father's full name SAMUEL RABSON
Occupation SALESMAN Birthplace RUSSIA
Mother's full name ROSE STRAUSS RABSON
Occupation HOUSEWIFE Birthplace BROOKLYN, N.Y.
Your spouse EILEEN KLIONSKY RABSON
Occupation ELEMENTARY SCHOOL TEACHER Birthplace BINGHAMTON, N.Y.
Your children THREE DAUGHTERS (ONE KILLED IN AN ACCIDENT) AND ONE SON.
Where did you grow up? NEW YORK CITY
Present community ROCKVILLE, MARYLAND
Education BS. AND PHD - CORNELL UNIV.
Occupation(s) RETIRED APPROX ONE YEAR AGO,
WORKED FOR GOVERNMENT IN RUNNING A RESEARCH AWARD PROGRAM
Areas of expertise PLANT SCIENCE,
PLANT PHYSIOLOGY
Other interests or activities ENCOURAGING IMPORTANT RESEARCH IN PLANT SCIENCES AND OTHER AREAS
Organizations in which you are active AMER. SOC. PLANT PHYSIOLOGISTS

Chapter 36

PETER E. YANKWICH

Arlington, Virginia

July 30th, 1996

VM = Vivian Moses; PY = Peter Yankwich

VM: This is a discussion with Peter Yankwich in the NSF Building in Washington on July 30th, 1996. It's actually not Washington is it? It's...

PY: This is Arlington, Virginia; we're across the river.

VM: OK, fine. Can I ask you what your career had been before you ever went to Calvin and how you got there?

PY: I graduated from high school in Los Angeles in 1940, went to Berkeley as an undergraduate, got a Bachelor's degree in 1943. In 1944 I started work for the Manhattan Project in hopes of keeping myself out of the South Pacific and I took my PhD in 1945. Just before that, it became apparent that the war was going to be drawing to a conclusion and everybody in the Radiation Lab. was sort of scrambling around, looking for other opportunities. Even before the actual end of hostilities, Melvin had this idea of his and I had started my graduate work with Samuel Ruben.

VM: Had you? I hadn't realised that.

PY: Sam was killed in an unfortunate laboratory accident and I finished my degree with Gerhard Rollefson. To make a long story short, I was already in the Radiation Laboratory and Melvin had this idea that he was selling to Ernest Lawrence. After the end of hostilities, he began to set up this group. I was still working for the Project (*Manhattan Project*) on various things and one day he appeared in my lab. and asked me if I'd entertain a reassignment to this new group that he was starting up. I asked him "why?" and he said "well, we're going to be doing a lot of work with radioactive carbon and you're the only person around here who has had any experience."

VM: What had been your experience with radioactive carbon?

PY: Well, in 1943, when I started my graduate work, I was doing carbon-11 work associated with one of the South Pacific gas cloud movement projects that Sam Ruben was working on at the time. After his death, a young instructor — my own faulty memory is going to kick in here — Tom Norris, who was working in the same

building, we worked in what was called the “Rat House” at that time, was sort of keeping an eye on me and he suggested that I work with Gerhard Rollefson on almost anything I wanted to. But he, Norris, had an interesting idea. That was he knew several years before Ruben and Kamen had created a barrier made of stainless steel tanks filled with solutions of saturated ammonium nitrate. This was their carbon-14 factory. He wanted...he suggested that I analyse the effluvia of these tanks, or get anything I could out of them, to discover what chemical form the carbon-14 was in and perhaps that would be an interesting research project.

VM: And you were, of course, a chemist by training?

PY: Yes; I was a physical chemistry undergraduate. So, I started to work on this. The problem arose immediately on how you counted all this stuff. At that time, all we had available that would really work was a counting system that was developed by Willard Libby for very low-energy β -emitters. I can't even remember what we called it, but it was made of a couple of standard taper joints and from one end came a screen — the anode or the cathode — the screen...it was called a *screen-wall counter*, I remember now, a screen-walled counter and there was this screen that was one electrode and the wire ran down the middle, which was the other electrode. The sample tube went over this array so that the particles from the inner surface of the sample tube went through into the active volume of the counter. And as you can imagine, you had to create the counter fresh for every sample, which didn't lend much to the statistics. You tilted it one way and got the sample away from the active volume, and that gave you the background, and then you tilted the sample over, usually losing part of it on the way, and so on. I had counted carbon-11 this way for the gas project and then I developed the technique for counting carbon-14 this way. While this was going on, by accident we stumbled on the small plate stratagem for counting these things, taking an alcoholic suspension of barium carbonate in water and evaporating it under infrared lamps onto a thin aluminium disc and then counting it with an end-window counter.

VM: When you say you “stumbled upon”, you invented it or came across it?

PY: I don't claim to have invented it, believe me, because I am sure this was a well established technique but nobody had ever used it to solve the problem of counting barium carbonate containing carbon-14. So when I started work with Melvin, my task was to count the carbon. There was no laboratory available to us, so I spent the first few months in a little cubby-hole on the third floor of Gilman Laboratory under the eaves splitting mica...

VM: ...to make your end-window counters.

PY: ...to make end-window counters. We tried all sorts of designs for the end-window counter. We knew from much bitter experience what the spacing of the grid had to be and all sorts of things like that to prevent instant implosion. I got to be very skilful at splitting mica. I remember my personal record was 0.9 mg/cm^2 which I thought was astonishing. But anyhow, we prepared these end-window counters and while everything else was going on in the laboratory, we were building up the apparatus necessary to use them routinely in counting. We built the lead houses to our own design, we had all these brass slides in and out, so on and so forth. Eventually we ended up on the second or third floor of Donner Laboratory, ready to go.

VM: Did you develop all the corrections for infinite thickness and variable thickness or was that known already at the time?

PY: No, it was not known and I have a paper, I can't remember who's on me with it (*sic!*), in *Science*, of all places, on backscattering and its relation to counting. We got the infinite thickness thing, we developed curves so that you could tell where you were on this and, therefore, you could use a sample of any thickness and so on and so forth. Because in the early days, sometimes we were working with very, very small amounts of material. The corrections for backscattering and coincidence came from one of the mathematicians...we called him a mathematician; he actually was an astronomer who was working as a journeyman mathematician in the Radiation Lab. on any problem that came up. It was he who developed the backscattering corrections that we used and the coincidence stuff came out of an article by Truman Coleman who was then at Carnegie University.

VM: You mentioned, but I missed it. You mentioned that Calvin recruited you from a project you were already working on.

PY: Right.

VM: What was that project?

PY: Well, when I was a graduate student, I was operating under an edict of that gentleman whose portrait you see there, that's my father and he was a federal judge. He wanted very much for me to follow him into the law and I wasn't interested at all. So, when I started to get ready to go to school he laid down certain conditions. One was that when he knew I was going to go into chemistry, I should go either to Berkeley or to CalTech, that a bachelor's degree was a triviality and that I should not stop until I got a PhD. And that it was his task to support me while I was being educated and I was under no circumstances to take remunerative employment for that reason. So, there I was.

Well, when the war came along, and I was anxious not to get drafted, the purpose of employment was not remuneration, it was to proof oneself against conscription. I am very sympathetically inclined towards Bill Clinton, by the way, for obvious reasons. So I figured that working at the Radiation Laboratory would be about the best thing. Well, I was a chemist and a lot of us were chemists and you can imagine the kudos that is available to a chemist in an organisation that is run by physicists. E.O. Lawrence lumped us with the janitors and other staff when he thanked everybody for their glorious war efforts. But, he learned.

So I became a shift supervisor, on rotating shifts — day, graveyard, swing — a horrible way to live in the analytical laboratory up on The Hill. The function of that laboratory was to prepare the samples for what we called α -counting, and it actually was mostly α -counting. What they did was, your colleagues from Britain were there in droves and they would do a run on the early calutrons and they would take these things out and they would take them down and every piece had a sample of the uranium that had spluttered all over the place sent to α for counting to see what the enrichment was and to find out where in the hell the stuff was going. Because when you take a mass spectrometer that normally works with submicrogram quantities of material and are building up to the point where you are trying to get a couple of hundred grams through it, you've got problems. So, that's what I was doing. I was supervising this laboratory.

VM: You also mentioned that you had worked with Sam Ruben. What were you doing with him?

PY: He was working on a defence project for, I guess, the Army. Bill Gwinn was a member of that project, Professor Giauque actually worked on that project for a while. Their task, as I understood it, and I never was very privy to useful information, was to devise a technique which would permit them to either predict, or if not to predict to then follow the motion of gas clouds in forests.

VM: You never worked with him on his photosynthesis work?

PY: No.

VM: Had Andy Benson gone by the time you were working with Sam?

PY: Yes. He was in spike camp.

VM: What about Martin Kamen, was he still there?

PY: Martin Kamen was sort of around but not around because this was the period of the witch hunt beginning. I talked with Martin only a couple of times and then mostly to find out what he could remember about the preparation of these tanks and other samples of high nitrogen concentration substances that I was analysing for my doctoral thesis.

VM: We talked to Martin a month or so ago. So, you were in Calvin's lab. then at the beginning of the carbon-14 activity which must have been at the end of 1945 or very early 1946. There were very few of you there at the time.

PY (*Jim*) Reid, myself, (*Bert*) Tolbert and Calvin were the group. The first addition to the group from the outside was Charlie Heidelberger and the second addition came from the inside and it was (I'm repressing his name. He's the other person who worked for Melvin whose father was a federal judge).

VM: Dick Lemmon.

PY: Dick Lemmon. My father and Judge Lemmon were old friends. They thought it was kind of hilarious that we two should end up in the same room. Then Al Bassham came along and that was the very early group.

PY: You were never part then of the photosynthesis activity.

PY: Not really.

VM: How long did you stay there at that stage?

PY: I wanted to go into academic work and in the spring of '46 I had a long heart to heart talk with Wendell Latimer and he said "I'll tell you what we'll do. You are a little young (I was 22 at the time) and", he said, "we've got to season you a little bit, so I'll make you an instructor". So, I was an instructor in the Chemistry Department in the academic year '47-'48. During that year, I got an offer to go to the University of Illinois and all I knew was that Roger Adams was a famous organic chemist and I didn't really know what an organic chemist was. You know Branch and Calvin weren't exactly organic chemists. T.D. Stewart was sort of an organic chemist in the Berkeley mould. I remember asking Rollefson whether I should be seriously interested in this offer; I had another offer from the University of Washington and one

from the University of Rochester. And he said "Peter, that offer from Illinois is the finest offer that one of our graduates has gotten in a decade. Take it." Ed King was in my class and he went on the same kind of advice to the University of Wisconsin. We had good advice. So I left in August of 1948.

VM: Having been there two and a bit years.

PY: I was there essentially for the first two and a half years of the laboratory's existence. During the last year of that I was teaching in the Chemistry Department a little bit of the time.

VM: Did you spend all your time developing the C^{14} chemistry and technology?

PY: Not directly. Melvin was very good to me because he knew of my interest in establishing an academic career of my own so he let me pick projects that were consistent with my interest provided they had some aspect that was of interest to him. The first one of these things had to do with beryllium nitride because it was a very concentrated nitrogen source. There had been a lot of work done on beryllium compounds for other reasons. It has a very, very low neutron cross-section; the beryllium has a very low neutron cross-section. I suggested, hey maybe we can get some really potent C^{14} barium carbonate out of beryllium nitride targets and, by dissolving beryllium nitride in various solvents, I can pursue my hot atom chemistry. So, he said "great". It was, you know, a mutual back scratching situation. He got his ultra-high concentration of C^{14} and I got more papers out on hot atom chemistry which was then my interest.

Then John Otvos and a guy named Wagner out at Shell Development did some work in which they demonstrated that if you decarboxylated malonic acid, there was an isotope effect. Melvin was absolutely fascinated by this. He said "How would like to take a look at this?" So I said "love to". I took a look at that and we did malonic acid, we did bromomalonic acid — which turned out to be a horrible disaster — and that started me on a career to which I devoted thirty years to kinetic isotope effects. I did some work on hot atom chemistry at Illinois but very quickly it became apparent that the isotope effect work was much more interesting.

VM: Did you see the beginning of photosynthesis in Calvin's lab.?

PY: Oh yes. Because Al Bassham and Dick Lemmon and, indeed, Andy Benson had been working on that. Benson just sort of came and went. He was the best connection with Sam Ruben's photosynthesis work. I was a very poor one because I never had been associated with it. Then, just before I left...not just; a while before I left, Andy came back. That was the start of the original heavyweight team on photosynthesis.

VM: From the standpoint that you had, working on isotopic carbon in Donner, were you party to the developments in photosynthesis even if you didn't work on it? Was it something which was discussed generally in the group?

PY: Oh, yes. There were all sorts of group meetings that we had. There were some personality clashes in the Chemistry Department. I really don't know what they were, but they had more to do with the internal politics of the Department than anything else. But Melvin tried various devices to get our research group sort of mingled with the other groups that were working, and vice versa, because he recognised the benefits of conversation and cross fertilisation as well as anybody did. Some of these were successful and many of them were not. People just weren't taking his work very

seriously and so on and so forth. The group itself, though, had all sorts of informal conversations. I don't recall whether we really actually had a weekly research conference of our own although I'm fairly sure we did because Calvin used to invite people from all over the campus to come up and talk to us about things that he thought we ought to know about and not necessarily have anything to do with photosynthesis or intermediary metabolism, or anything like that.

VM: So he was a good, stimulating leader?

PY: Yes, absolutely, absolutely. And very catholic in his tolerance. I felt that I was able to do really anything I wanted to. My formal duties were exceedingly clear and kept to an absolute minimum and I was asked to bring to the group such additional expertise as I acquired but I was not pointed forcefully in particular directions in order to acquire it.

VM: In those days also, was there adequate funding for anything you needed to do? Was funding an issue?

PY: I was not aware of that. This was all handled by Melvin and it was an interaction between him and Ernest Lawrence.

VM: You weren't enjoined to save money and be careful and things of that sort?

PY: No, not that I'm aware of. If we needed something, we got it. If we needed something built, it got built. The shops and the glass-fabricating facilities in Chemistry, in Physics and in the Radiation Laboratory itself were right there, there to be used.

VM: Socially, among the group at that early stage when there were just a few people, were you socialising between yourselves, out of hours, at the weekends?

PY: That was largely Tolbert's creation. Tolbert very early in the process became an administrator of the group and everybody was delighted to let him do it. He was very good at it. He thought we really should see each other outside the laboratory so there were picnics, and thing up in Tilden Park and so on and so forth, occasionally. The group did interact on the campus.

PY: You were all very young at the time.

PY: As I look back on it now, we were damn young!

VM: But largely unmarried, were you, and without domestic responsibilities?

PY: I was, I may have been the only one in that original group who was married. That was also one of my father's dicta: "Thou shalt not marry until thou hast thy PhD".

VM: Did you follow that?

PY: Oh yes. I got my PhD in June of 1945 and my wife and I were married on Bastille Day in 1945...

VM: Congratulations!

PY: ...and we're still married!

VM: You've just celebrated an anniversary.

The other question I wanted to ask you: did you see the dawn of the occupancy of ORL by Calvin's group? Did they take the building while you were there?

PY: Yes, they did. This was an interesting occurrence because as the need for the analytical laboratory had decreased, I began to get other assignments (*at the Rad. Lab.*) and one of them was to work with a man who was in the Chemistry Department at UC Davis who was down at Berkeley. He was working on a device to move, I think, uranium oxide along a tube so that it could be fluorinated and they were using what was then new in this kind of operation, it was one of these devices that just vibrated everything. You tuned it properly and the uranium oxide would go moving along. So we used to...this was all done in ORL; this was before I worked for Melvin. While that was going on, I had an assignment to Melvin's group; this was before he asked me to join the Bio-Organic Group. That was to use some of the small hoods in Gilman Laboratory and build an apparatus for synthesising some fluorinated compounds that he wanted. I designed the apparatus and it was all built out of stainless steel and so on and so forth, and I still have a place on my thumb where I burned off the top of a thumb with HF. I was working on this for a while and that's really where Calvin got to know me. It was after that, as that project began to wind down, that the Bio-Organic Group became crystallised and started up.

VM: And how does that relate to ORL?

PY: It doesn't relate to ORL. My work for Melvin was done in what later became his office in the Old Chemistry Building, the place with the fireplace. At the same time, I was working with this guy from Davis over in ORL. A few months later, when the Bio-Organic Group had been established, that space (*in ORL*) became available to Melvin. Here I was, back in space that I knew from a different effort, working on different things. This was about the same time that we were in Donner.

VM: I know, of course, that you have been in the round building because I have seen you on the movie on the 1989 reunion. You no doubt know the philosophy of that round building.

PY: Yes.

VM: What do you think? Successful do you think?

PY: I don't think one has to guess at its success. You just look at the output. It was completely consistent with the way Melvin ran his own science. It was lots of conversation possible, people being thrown together, a building laid out so that there were many intersections of trajectories and you couldn't get from here to there without running into somebody else.

VM: You liked it?

PY: That's a fairly interesting...I'm not saying I like that, but it was a very effective device.

VM: Have you maintained much contact with the Berkeley group?

PY: None. No, when I left Berkeley the people with whom I maintained contact over the years were a small group of people who, like myself, became its alumni and not

necessarily of the group itself. I kept in touch with Dick Lemmon for a variety of reasons — our parents both being federal judges, Dick got interested in American Chemical Society affairs, he and I were both on the Board of Directors of the American Chemical Society at the same time. I kept in touch with Bert on a very occasional basis. I knew where Bert was and what he was doing. I lost track of Jim Reid after he came back here to the NIH and I wasn't aware until several years afterwards that he had passed away.

VM: Melvin? Did you see Melvin?

PY: I saw Melvin occasionally. I would run into him, mostly at ACS meetings. You know, we'd have a five minute chat and that was it. One of the people with whom I kept in touch, I visited only once after he left Berkeley, and he has since been knighted, I guess, and that was Ted Abraham who became the head of the Sir William Dunn School of Pathology (*at Oxford*). Ted had arrived about a year before I left, I think, and we corresponded in a desultory fashion over the years. In the middle sixties I had occasion to go to Britain on my way to an international meeting in Dresden of all places and I walked by this place and I saw School of Pathology. I walked in and said "Is Professor Abraham here?" People sort of looked at me: "Why yes, he is." So I said "Could I see him?" And that was it.

VM: We wrote to him but he hadn't had a chance to reply before we left England. So I hope still to see him. What happened to you, you wanted an academic career and you got one?

PY: I wanted an academic career and I got one. I led a very simple life, a very simple life in comparison with some of my colleagues who became what I would call "academic vagabonds". That's a marvellous way of advancing one's career but I was extremely fortunate that I didn't have to do that. There came a point in my career where I accelerated its advancement by getting offers from here and there and so on and so forth, and I was fortunate in that the people who were successive heads of the Chemistry Department at Berkeley (*Editor: this presumably should be Illinois*) were always willing either to match the offers or sometimes meet them half way and sometimes go beyond them. I arrived in Urbana in August of 1948 and I left Urbana, not knowing I was leaving, in October of 1985 to come here. In the interim, I worked my way up through the ranks and I was not one of these people to whom research was the be-all and end-all. I kept the work on hot atom chemistry going for about five or six years, I did isotope effects for most of the rest of the time, and I never had a research group that was larger than four people, including one or two postdocs., and I never wanted more. I felt that with more I wouldn't know what was going on and I felt that I had to know what was going on if I was going to give guidance and train people and assist their education.

So my avocation became university politics and for many years I was chairman of the Committee on Committees of the Academic Senate, which is the kingmaker's role, and I was also the University's representative to the Illinois Board of Higher Education which was where the dirty politics in a system as complex as Illinois as we have five university systems in the state. I enjoyed that tremendously. In 1977 I joined (*Jack*) Corbalee, who was then president of the University. He wanted someone who came out of the faculty to help him solve his problems and I became Vice President for academic affairs, never having been a department head, a school head, a dean, a vice chancellor or anything. I went from professor to vice president, and it was more fun than a barrel of monkeys. Jack Corbalee left the presidency about three years later and his successor, Stanley Eichenberry, came into office and we

tried very hard to get along with each other and there were just a lot of ways in which we couldn't. So one day I said "I think it's time for me to go back to being a chemist". He said "that isn't the way I've got it set up". By this time, John Corbalee had left to become the head of the MacArthur Foundation in Chicago. He said "how about your moving into Jack Corbalee's chair in the School of Education, the College of Education?" I said "Stan, can you see anybody on this campus taking *me* seriously as a member of the faculty of the College of Education?" He said "oh". So he said "I'll talk to Ron Brady and maybe we can move the money around and put you back in chemistry". So what happens? I go back to chemistry in the most unusual of all situations. I came on a line that was created for me in the budget. I was untouchable. I didn't have to do anything. My wife used to say "Peter, you are not being paid to be a professor of chemistry. You are being paid to be not vice-president!" She was right. I had all through these years as vice-president — all five of them — I'd had postdocs. working with me, thanks first to the Atomic Energy Commission and later to NIH. When I went back, I decided I've got to get back up to speed in my research so that I know as much as the postdocs. know. I was really fooling them. I knew how to ask questions, you know. I'd had 35 years of asking tough questions and I could do that to cover a lot of things I didn't know. The fact that they didn't know them either was part of the game. But anyhow, so I went back to chemistry and I taught my classes and got myself all revved up in my research again and was enjoying life and fulfilling one of my lifelong obligations, which was to study Attic Greek. When I was in high school I took four years of Latin. My father was a superb linguist and I have always had an appreciation for language and I realised very early in my college career that there was a deficit in my mind, and possibly even in my character, that I had never given myself the opportunity to learn Greek. I always swore that whenever I retired, or had time, I would learn Greek. When I stepped down from being vice president was the perfect time. I went to the head of the Classics Department and I said "would you mind very much if I sat in on your beginning courses in Greek?" He said "absolutely not. You'll be a good role model". It was fun. I sat in Greek courses for three years.

Then an old colleague of mine from Chemistry was called down here to head the Education Directorate...

VM: "Here" I should say for the tape, is Washington.

PY: ...of the National Science Foundation. He came down in 1944 (*sic! 1984 seems more probable*), this was (*indecipherable*) Shakashiri, who is an inorganic chemist. He was enjoying a very interesting career down here and he needed some political-administrative help. So he asked me to come down. I came down in 1985 for a year, stayed another year. After I had been here nearly three years, I figured, "look, I can do the world much more good staying right here than going back to Urbana and doing chemistry again". So, I retired from the university and stayed here. That's where I have been ever since.

VM: Last, last question: what did being in Calvin's group do for you?

PY: What did it do for me in what way?

VM: Professionally.

PY: Professionally, very little because I moved in circles where Calvin was known, especially later on as the person who had unriddled photosynthesis. But his research interests, even though he got his degree with George Glockler at Iowa...no, at

Minnesota, I beg your pardon...Glockler was at Iowa, Melvin came from Minnesota...even though I had been associated with Melvin, I was not associated with him in the areas that were important to the career that I was building. Gerhard Rollefson as a sworn and acknowledged physical chemist was far better known to my colleagues at Urbana than Melvin was. No, my profit from the years with Melvin was that he gave me an invaluable opportunity to become seasoned. I could experiment, try out things in an absolutely fail-safe situation, I was part of a group that was made up of very bright, very gung-ho young people who were working on interesting things and, even though I wasn't working in the main currents of that work, I was aware of it, I saw the intellectual and professional activity that was necessary to keep it moving. I learned what you had to do to be a successful researcher and administrator of research. That has stood me in excellent stead, all of my life.

VM: That's a legacy worth having.

PY: Absolutely, and I'm very grateful for it.

VM: At this point it remains only for me to thank you because you have told me that you have to leave very shortly.

PY: I should leave shortly.

VM: Thanks indeed for your time and for telling us what you have.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name PETER E(WALD) YANKWICH

Date of birth 20 OCT 1923 Birthplace LOS ANGELES, CA (USA)

Father's full name LEON RENÉ YANKWICH

Occupation FEDERAL JUDGE Birthplace JASSY, ROMANIA

Mother's full name HELEN ANNA CORNELIA WERNER

Occupation PIANIST Birthplace EAU CLAIRE, WI (USA)

Your spouse ELIZABETH POPE INGRAM

Occupation ADMINISTRATOR Birthplace SO. PASADENA, CA (USA)

Your children ALEXANDRA HELEN (CAPPS); LEON RENÉ II;
RICHARD INGRAM

Where did you grow up? LOS ANGELES, CA

Present community ARLINGTON, VIRGINIA

Education BS., PH.D. UC-BERKELEY (1943, 1945/46.)

Occupation(s) EXECUTIVE, NATIONAL SCIENCE FOUNDATION (CURRENT)
PROFESSOR OF CHEMISTRY (1948-1988) U. ILL.-URBANA.

Areas of expertise EDUCATION, PHYSICAL CHEMISTRY, HIGHER
EDUCATION GOVERNANCE

Other interests or activities MUSIC, DOGS, ASTRONOMY.

Organizations in which you are active AM. CHEMICAL SOCIETY, AM.
PHYSICAL SOCIETY, SIGMA XI, SIGMA PHI SOCIETY.

Chapter 37

E. MALCOLM THAIN

London

November 15th, 1996

VM = Vivian Moses; MT = Malcolm Thain; SM = Sheila Moses

VM: This is November the 15th, 1996 and we are talking to Malcolm Thain at Queen Mary College in London.

Malcolm, let's start with that card you just showed us, a card that they gave you when you left.

MT: That's right.

VM: What sort of memories does this bring back to you?

MT: You know, thinking the last week or two, my general impression of the laboratory, it was an extraordinary energetic and happy place. I think people liked working there. The trips out to the Sierras, the camping, the weekend activities — so that everybody was living close by and so it had a splendid social structure to it. Having said that, I think that Rod Quayle and I, as we got to know people better, realised that there were undercurrents which we, as British people, didn't fully understand. For instance, quite a number, we understand, of people actually went to psychiatrists because of tension at work.

VM: Did they?

MT: Yes. I don't know if I could identify (*their names*) on the card. You'd have to remind Rod of this. We found one of our colleagues in the lab. saying "haven't you got somebody to advise you? Haven't you got a counsellor?" Why, we were absolutely astounded. No, we hadn't anybody to give us advice on this.

VM: You came from a "stiff upper lip" country.

MT: That's right. But, you see, a different culture. That was just a very minor sort of undercurrent which was rather strange to us. But generally, a very lively, happy place to work. And, of course, the social life was so integrated with the laboratory life.

VM: Was your wife there with you?

SM: Yes, I can. Would you like me to?

MT: Yes.

SM: ...he is wishing you all the best and strength in your work.

VM: Has he signed it?

SM: It's the same person who wrote this: "God save the King (*indecipherable*)".

MT: I don't know who that is. Who would have been writing in Hebrew?

VM: Was there an Israeli there at the time? We'll have to look up the records and see what we can find.

MT: This one also I can't read.

SM: Well, it's someone whose initial is W.

MT: Iso or...

SM: Something...it's hard isn't it?

MT: This was given to me in a book. When we left, they presented me with a book of Yosemite and the Sierra because they knew how much we had enjoyed, you know, camping and walking up there.

VM: Did you have a going away party?

MT: Yes, indeed. That was held at a park near by as a barbecue.

VM: Ah, yes, in the style of the times.

MT: In the style of the laboratory, in the style of the times, yes, yes.

VM: Can we back-track and ask you how did it come about that you went there, to Calvin's lab.?

MT: I think, in fact, I was working in the early '50s in the Lister Institute of Preventive Medicine with Jim Baddiley. And we worked on the structure of coenzyme A.

VM: In that case, I need to back-track even more. What was your training background?

MT: I did my PhD under Jesse Kenyon on organic chemistry, the mechanism of carbonium ion reactions. When I finished that, which was mechanistic chemistry, when I'd finished that, I said to Kenyon that I would rather like to have a hand, or try a hand at natural products chemistry. He knew Walter Morgan who was head of the Biochemistry Department at the Lister Institute and so I joined a small group, in fact joined Jim Baddiley when he joined the Institute. We had several stabs at things and one of them was the structure of coenzyme A in 1949; that's the sort of main line.

VM: That would have been early '50s?

- MT: That would have been in '49. I worked there for several years on, amongst other things, an ICI Fellowship and then Grant Buchanan joined who had known Jim, of course, in Cambridge. Grant Buchanan told me about Berkeley and when I wanted a change of laboratory, and Jim was going off to Newcastle, I thought that I would apply to go to Calvin.
- VM: Of course, Grant had already been there...
- MT: He'd been there and come back. And so it was through Grant Buchanan that I knew about Calvin's laboratory. I got a Royal Society Natural Sciences Foundation fellowship for a year.
- VM: Did you make any arrangement with Calvin as to what you would do when you got there?
- MT: No, I just turned up with a suitcase and umbrella much to the amusement of the reception committee.
- VM: Did you often use your umbrella?
- MT: That was Grant again. He said that an umbrella was actually a very useful thing to have in Berkeley. I think the advice, which was individual, certainly created amusement!
- VM: You went with your wife?
- MT: Yes.
- VM: How did you travel? This would have been early '50s was it?
- MT: It was a National Sciences Foundation...anyway, part of the finance came from the US and they arranged the travel. We went over on the *Ile de France* and that was a splendid experience. Then we went overland by train from New York to Chicago to Berkeley. When we got off the train we were met by, I think, Ning Pon and Al Bassham.
- VM: Really; on that station?
- MT: At the station, yes, and Rod Quayle. We stayed in fact, in an apartment near Rod Quayle for a year or two...well, for month or two and then we both had an apartment on the hill, opposite the architectural building, the School of Architecture...what's the name of that hill going up the side of the campus?
- VM: Well, there's a road called Hearst on one side and Bancroft on the other side.
- MT: I think it was Hearst. We both lived in an apartment house on Hearst.
- VM: That was very convenient.
- MT: So we just walked through. The amusing thing was, it's a very steep hill, and so all the lorries used to change gear and you were never quite certain whether it was a lorry changing gear or a minor earthquake.
- VM: Just outside your bedroom window?

MT: Just outside our bedroom window. There were several occasions when there were minor earthquakes as, of course, the campus sits on a fault.

VM: Can you remember day one, when you first went into the lab. and when you first met Melvin, presumably?

MT: That would have been the first occasion of meeting Melvin. The actual going in, I don't actually remember: no specific memories of the first day.

VM: Or when you first met him?

MT: It must have been very early on because the first person I think we met was Al Bassham because he saw us at the station. And Melvin shortly after. I have no specific memory of that very first meeting. Later on, of course, one formed impressions of how I he would come through in the morning, full of energy and enthusiasm, to see what was going on, and how later in the day he had spent his energy so he would come through a bit deflated, you know, needing revitalising by some new, interesting results.

VM: How did you decide what you were going to work on?

MT: I was only there for a year. I had a leave of absence from my fellowship here in London and so I really fitted into the laboratory. I didn't go... I had read, of course, the papers about the work they were doing and I wanted to do something on C¹⁴ tracing work...

VM: To learn the techniques.

MT: To learn the techniques. That was the idea of going over there. I was happy to fit into the existing programme.

VM: And what did you actually do? Which bit was yours?

MT: I was looking, actually, I think it was to see if there were any complications in the phosphoglyceric acid take-up of C¹⁴, the usual techniques of using plates and chromatography. Of course, Bob Rabin found the addition of CO₂ to the ribulose. There was no hint of that when I was there.

VM: So the mechanism at the time when you were there, the carboxylation mechanism, was still obscure.

MT: Yes.

VM: Did it break out, did it develop while you were there?

MT: No, I'd say after. In fact, one of the things I was doing, towards the end, was incorporating deuterium into compounds to get the NMR spectrum.

VM: And who were the people who were working on the cycle dynamics at the time when you were there: names that come to mind are Peter Massini but I think he must have been earlier than you.

MT: No, that name is not familiar to me.

VM: Alex Wilson?

MT: Alex Wilson; he had left just before I arrived. I saw his papers, and there's that famous paper of his with the little man with the fishing rod; that was just before I arrived.

VM: We have talked to both the author and the artist.

MT: So the people — Andy was working there for a time, but he then departed half-way through the year to go to the east coast. He departed with his family and his whole tribe of bonsai trees. He was a very keen horticulturist and so I think he sent his family on by train and he went with the car and trailer with these trees which he watered at intervals as he went along.

VM: So you were there when Andy actually left?

MT: Yes.

VM: Was it anticipated, was it sudden, what was the mood in the place like at the time when Andy was leaving?

MT: I think that...Of course, I wasn't privy to the inner circle discussions, but it did come somewhat of a surprise to me and I think it did create a bit of a vacuum to start with. He was missed. Of course, there was Al Bassham there who was sort of continuing...and Bert Tolbert, too.

VM: Did Andy leave shortly after saying he was going to leave, or was there a long period or a short — do you remember?

MT: I don't think I ever knew.

VM: But from the time that *you* knew, he was virtually going, was he?

MT: I would say — we got there in November and I would guess he left in the spring, I think it was all accommodated fairly quickly.

VM: And so, as you say, there was a vacuum. Take a long time to fill it, did you think?

MT: The dynamics, I think, of the laboratory were very dependent on the great range of scientists coming into it. I think this is one of the characteristics of it. When I was there, there were people from France (Jean Bourdon), from Norway — I forget the gentleman's name...

VM: I forget it: Arnold...(Editor: Nordal).

MT: Certainly from Japan (Kazuo Shibata), myself and Rod (Quayle) from England, Switzerland, a Swiss fellow: so that all these people coming and going, spending a year or two and I think that the contributions these diverse backgrounds made to the programme were a bit incalculable in advance. But I think Calvin so of used, or responded to these people and their expertise.

- VM:** Calvin and Benson were certainly very close to one another and they had set the thing up together and they were the driving force. When Andy had left, was Calvin's interaction with everybody else any different from what it had been?
- MT:** No, I wouldn't say so. I think he probably relied on Al Bassham for continuity. My recollection is that Calvin would come through the laboratory and speak to everybody, so that it wasn't, you know, as if he had a mouthpiece in the laboratory. He would come to everybody and walk through talk and hear what you had been doing, making suggestions and, of course, there were the seminars, too, which were held in the Chemistry Department, with Rapoport and...
- VM:** At eight o'clock on Friday mornings.
- MT:** That's right; that's it. You remind me now.
- VM:** I notice you smiling. You have the usual English attitude.
- MT:** That's right; I'm well adapted. Calvin's interaction with all the group, I'd say...
- VM:** I know for a fact, and was later part of it, that there was, of course, a management in the group, that the post-docs. were not part of the in-house technical management administration. But I didn't get the sense, all the time, that there was an inner coterie of scientific decision-making. I felt, really as you said just now, that it was an open decision-making thing and it was done all the time with everybody.
- MT:** Yes, I would agree with that. I certainly didn't get an impression of a group of two or three people with Calvin in it formulating policy and ideas. I would say that's absolutely contrary to me understanding. It was very open. In fact, I thought the graduate students we had, for instance, Ning Pon was a graduate student at that stage, I wasn't aware that they weren't treated as equals. They were in the same laboratory and they might be just the other side of the bench.
- VM:** You worked, of course, in ORL, on the main floor in one of the big labs. People have offered the proposition that the building itself was a factor, the structure and layout of the building was a factor in the way the group developed.
- MT:** Yes, I think it probably is. It was compact and the place where you had coffee (of course, we were allowed coffee in the laboratory in those days; unthinkable now) — Altha used to make coffee in the corner there and try to imitate our English accents and we used to imitate hers — the laboratory benches were all clustered around that centre so that there was a community and you interacted with people everywhere in the laboratory.
- VM:** Have you ever worked in as open a lab. as that, anywhere else?
- MT:** Certainly not in University College. When I was in the Tropical Products Institute we had open laboratories there as a matter of policy so that people did interact. Also, I think we felt that if you put a person in as laboratory by themselves, the isolation is really not productive to really the best work. We definitely went for open laboratories of that style. I can't say we chose them because I worked at Berkeley; no, definitely I didn't choose them because I worked at Berkeley. I think they chosen by a fellow called Stuart Stainsley who had worked at Mill Hill. He was ahead of me in the Tropical Products Institute and he was the director.

VM: When you went to the Tropical Products Institute you already found these open labs.

MT: Yes.

VM: ...and they seemed natural to you.

MT: yes.

VM: As you say, everybody mucked in and there was no sense of hierarchy in these labs. at all. The building seems quite archaic by modern standards but it did have lots of advantages.

MT: One of the things that I do remember — of course, you had to go down to the cellar to do the counting, the Geiger counters were down in the cellar. At one stage I was doing an enormous amount of up and downing, taking out plates, counting, changing them. At the end of the day I was quite tired from doing all this. So I calculated out how much energy I'd, just raising myself up from that cellar to the laboratory floor level n times. It came out to be a 2 oz. bar of chocolate at which I felt thoroughly abashed!

VM: That's all?

MT: That's all!

VM: That early style of counting, with the manual placing, was a bit tedious.

MT: And, of course, the fragility of the mica plates. It was quite an art, making the...splitting the mica plates to make them thin enough.

VM: You were of an era when the mica was split. By the time I got there which was...

MT: You were using mylar; mylar was coming in. Mylar was coming in, but we still used the mica plates when I was there.

VM: Which year did you arrive?

MT: It must have been the autumn of 1954.

VM: OK — and you stayed for essentially a year?

MT: Yes.

VM: In the interim, between the time you leaving and a year later when we arrived, mica had gone and mylar was in. That particular problem we didn't have. It must have made an enormous amount of difference. So you really had a lot of trouble with those mica end-window plates?

MT: I wouldn't say a lot of trouble. One recognised their fragility but they were no more difficult than other things. I suppose the advantage of the mylar was its availability. You see, mica of that size...these counters, you will remember were about 7 cm. across.

VM: Some still exist.

MT: Well, you see, mica of that size and regularity is really a rarity, I should think.

VM: Did you split your own mica?

MT: No, I don't think I did. I suppose somebody like Bert Tolbert did...

VM: Oh; it was done in the lab.?

MT: I get the impression it was done in the lab. I can't be certain of that but I got the impression that it was done in the lab.

VM: So every now and again you would be using one of these end-window counters and you would realise the mica had got a hole in it or whatever.

MT: That tragedy never happened to me but it happened to some.

VM: So you don't know what the mechanism would then have been for dealing with that?

MT: For dealing with that? No. Bert Tolbert might know. Al Bassham would know, too.

VM: It is just that nobody has ever mentioned that (*Editor: but see the interview with Peter Yankwich*) and it never occurred to me to ask. I'll get back to them about this. You think the mica must have come in reasonable size sheets, which they had to split?

MT: Yes. They split it up so that it was fine enough to give, you know, good counts.

VM: And they had to glue it into place, I expect.

MT: And, of course, then the gas was led through the chamber.

VM: When you got there in '54, the chromatography was already well in hand; the whole technology was...

MT: That was well developed.

VM: And there was that very smelly room upstairs under the eaves.

MT: Yes, the butanol and acetic acid; that's right.

VM: Got your dose of liver poisoning.

MT: Fortunately, butanol isn't so bad.

VM: The bulk of your work, then, was devoted to the PGA problem. I don't remember your papers, I have to confess.

MT: In fact, I don't think it...the point that Calvin was looking for was an unusual distribution of C^{14} in the PGA and it wasn't there. So I don't think it ever led to a publication.

VM: Did you not publish from there?

MT: I think the only thing published from there that year was on the early stages of NMR. So as far as I was concerned personally, it was not a productive time for publication.

MT: He badly wanted to have a pee but he didn't know the other so well and had the usual English reticence. So he said "that's a wonderful place to photograph — let's take a photograph there". Somebody said "it's much better later on". He was dying for a pee. They said "it's much better to take a photograph later on." When they finally did let him out of the car to take a photograph, he couldn't get his zip down and he swore he would never have zips again; he would always go for buttons! (*Laughter*)

VM: Well, you've left us without the denouement. What happened?

MT: Well, I think he did actually get the zip down but Rod himself didn't develop that too well.

VM: No. He didn't tell us that, actually.

SM: Was Yehuda Hertzberg there, could he have been the Hebrew? Because he was there...

VM: Hirschberg.

SM: Yehuda Hirschberg.

VM: I rather think he came at the same time we did, but I'm not absolutely sure.

MT: It doesn't ring a bell.

VM: I'll have to look up the list and see who might have been there from Israel at the time.

MT: You asked who were the main links. I mentioned Rod Quayle and he was working on the adjacent bench. Of course, Al Bassham had really taken Andy Benson's place and he was the person who knew the background if you had any problems on supplies or discussions. He was the person who knew the techniques, supplies and things about the laboratory. Bert Tolbert, to a certain extent, but my links with Bert weren't quite so strong.

VM: He was in Donner, of course, together with some of the other people. I noticed that your card has been signed by people who were in Donner.

MT: Marguerite and Dick (*Lemmon*)...

VM: ...and Rosemarie (*Ostwald*) was in Donner and maybe some of the others.

VM: Martha, I think, at that time was in Donner as well.

MT: Might well have been.

VM: She was working, I think, with Ed Bennett but I'm not sure.

MT: Yes, Ed Bennett — you mention Ed Bennett but he's not down here, is he? I wonder unless I can...

VM: There are names all over the car, aren't there, and it's difficult to spot them.

MT: Clint (*Fuller*); now, of course, that's another name. He left about the same time, I think, or a little after Andy I should think.

VM: So he overlapped with you in the lab.?

MT: Yes, he was there in the lab. He was really in charge, as I remember it, of looking after the algae collection; and he was (*taken?*)...who was it? Norris?

VM: Rich Norris.

MT: Rich Norris. Yes, it's Rich. When I see a Rich here (*on the card*), I think it must have been Rich Norris, a very nice man indeed, I liked him very much and he was always very helpful.

VM: I have never met him and I have an idea he died; I'm not sure whether he did. I'm getting a bit confused now about what we've been told. His wife, whose name was Louisa...

MT: Yes, Louisa, that's right.

VM: ...who was in charge of the algal (*cultures after Clint Fuller left*) and, I think, worked with Altha to some extent on the algae. She lives somewhere to the east of Seattle and when we were there, she simply couldn't get to us and we couldn't get to her; it was some hours drive away. I know about them (*the Norrises*) but I have never met either of them. Of course, Rich Norris is in your deerstalker photograph.

MT: That's right, yes. In his quiet way Rich was very able, providing the *Scenedesmus* cultures — invaluable. I do remember thinking at that stage what can easily happen is when you have a group like that, which are mainly chemists-biochemists, you bring in a biologist and, if he is, say a soil (?) botanist, it is very easy for him to lose his botanical sort of basis and become one of the others. I must have discussed that with Rich Norris because at the back of my mind there is that sort of thinking and it's probably from speaking with Rich Norris.

VM: Was Rich a biologist?

MT: I think he was a botanist.

VM: You think he lost his botanical flair?

MT: I think we were discussing the hazard in that situation of losing it. You've got to take positive steps to keep it, to keep your identity, your discipline identity so that you can make your contribution.

VM: You think that a botanist would wilt under the pressure from all the chemists rather than influence them?

MT: I think it's a possibility. I think also Clinton (*Fuller*) had the same feeling. Have you spoken with him?

VM: Yes.

MT: Do he never feel that he might become less of a biologist in that situation?

VM: I can't remember that he said so but I have to say that we heard so much from so many people in a short time that until we go back and play them all over again, I really won't remember.

When you went there (*to Berkeley*), you told us that you had already had the experience of doing a PhD in England and you had been working in the Lister for a bit. What differences did you find in style between your previous experience here and going into this American context? Do you think what you experienced was American or Calvin's group?

MT: I think at that stage in the '50s there was an international approach to laboratories. There wasn't all that great difference between the laboratories I had worked in UK and in America. I can believe that certainly in some UK laboratories, they were still very patriarchal but I hadn't actually worked under such a regime. To me, there was no great revolution or revelation. I had always been in close touch, daily conversation, with the head of department, whether he was leading me to a PhD or with Jim Baddiley working on coenzyme A, so that there was no great change there. I do think that any head of department stamps their individuality on a group. And I think Calvin stamped this conviviality that I mentioned before. So that, if you work in London, the chances are at the end of the working day, you spread to all quarters of the compass. Social life is made that much more difficult. Whereas in Berkeley, you were really within sort of walking distance of one another. This was the great enjoyment that we felt there. The social life, the conviviality, the friendliness which spread from the laboratory through to personal activities.

VM: And the convenience allowed you to pop in at all hours of the day and night.

MT: Yes, that's right. You could just go across (*the campus*) and continue something just half an hour in the evening and it means you got 24 hours more time in.

VM: Was your wife working while you were there?

MT: No, she's a teacher, and she didn't have a work permit so she couldn't teach professionally, but she used to help with one or two of the families round about who had young children.

VM: You didn't have any children at the time?

MT: No.

VM: You were able to go in and out of the lab. as often as you needed to.

MT: Yes.

VM: I remember it was the sort of place that people would come in at odd hours, to put chromatograms on...

MT: ...that's right, or change counters or something like that. It was just the guards. Perhaps that was the thing that did surprise us, the guards with pistols in the holsters.

VM: The campus police.

MT: Yes. That was a novelty.

VM: You remember there was all the palaver about getting clearance to work in the building.

MT: That's right.

VM: For UK citizens there wasn't any problem but for some of the Eastern Europeans, I think they were simply not allowed to work there. It was a long time before they got any...

MT: I know that to get clearance was important. They actually did keep tabs on you. I was very surprised because, you see, we had a permit to go in and when we left we went via a Southern Route through New Mexico down into Mexico. We crossed the border at El Paso. That was an amusing occurrence because we were about to go through on a bright and sunny day when suddenly the sky became dark and I realised there were four enormous military police stationed just round me. I was wearing, for the convenience of travelling, an ex-US Army haversack. They said “Hi, bud, where do you think you are going” in their inimitable southern drawl. I responded with one word which was sufficient: “well, actually...” and at that they pealed with laughter and I was let through. We left El Paso on a certain date, which is in my diary. About four years later, in the UK, I received a letter from the US Immigration Department saying they had no record of me leaving the States and could I provide them with details. I thought what an extraordinary machination of paper, going on so long. In fact, because of my diary I was able to supply them with time and date accurately.

VM: So you went into Mexico and came back to Britain from Mexico?

MT: We went down through Mexico and flew from Mexico City to Cuba and from Cuba to Jamaica and from Jamaica on a banana boat home.

VM: I see. Well that was an interesting route to travel.

MT: Yes, Greyhound, you see. We went by Greyhound bus all across California down to Mexico City. We thought “oh, splendid”. We didn’t realise that there was a quantum change between the Greyhound in California and the Greyhound in Mexico! Very interesting experience.

VM: What sort of contact have you kept up with the Calvin group since you left at El Paso?

MT: I saw Calvin several times when he came to the UK, when he collected an honorary degree at Nottingham. When some people like Al Bassham came over — saw him. Ning Pon; Ning Pon, of course, came over several times, sometimes unexpectedly. On one occasion in Sheffield he turned up in our drive in a camper. He and his family. On another occasion on a Broads holiday, I wasn’t present at the time. Again, on the edge of the water, a camper arrived and there was Ning Pon waving to my family in the boat. We exchange Christmas cards but that’s about all.

VM: Have you been back to Berkeley?

MT: Yes, I went back several times. Because I used to sit on recruiting boards for the British civil service, finding jobs for British scientists who wanted to get back to England. And so I used to interview, was one of a panel of interviewers and San Francisco/Berkeley was one of our stations. So I think of two or three occasions I went back there.

VM: Presumably you saw people when you did?

MT: Indeed, yes.

VM: On some of your later visits, the new building was there?

MT: The new building was in operation. Yes, I saw that; the round building.

- VM: I am sure somebody must have told you about the philosophy that went into the design of that building to replicate ORL. How did it strike you? You know, one can never recreate an environment because it's got so much...
- MT: I think that my impression, and this must only be taken as an impression, is that they tried to recreate something which was small and enlarged it and retain the quality. I think the enlargement, actually, led to a loss of a particular quality and this was the impression I got from the people speaking there. That's just an impression.
- VM: It's a difficult situation. How else would you have designed a new building? I was part of the design team and remember the difficulties we had.
- MT: The challenge.
- VM: But ORL was rather a unique structure which we subsequently found was built in 1885, much earlier than most people thought.
- MT: It was really a wooden structure, wasn't it?
- VM: It certainly was a wooden structure.
- MT: I can remember, we used to watch with amusement: you remember, out by the greenhouse, there was the laboratory dustbin, and high school students used to come along there surreptitiously and riffle through the dustbins to get out bits of equipment which they could build into their own experiments. I don't know if you remember that or not. I can remember the ambivalence towards this, thinking that the dustbin might have something dangerous in it but, on the other hand, these eager young scientists could have the encouragement of finding bits and pieces which would be useful to them.
- VM: You didn't put out a tray of non-dangerous things?
- MT: It wasn't sort of systematised such as that.
- VM: It was rather an interesting building. I think...I'm trying to remember back myself. It didn't strike me as being that interesting when I first went into it. It's only, I think, with the passage of time that you realise...
- MT: And also the comparison with other laboratories in which you worked.
- VM: I both went to and came back from much smaller rooms than they had there and so I was impressed at the time. And then I went back to stay for a long time in these big room environments. In the interim, when the group was in the Life Sciences Building, it was very different. There was one big lab. What I meant to ask you earlier on (I remember now): there was, of course, the group in Donner which was separate from the ORL group — Bennett, Lemmon, Tolbert— were you in close contact with them?
- MT: No. I knew them and they attended the seminars. In fact, I did a bit of work with Bert Tolbert. They certainly took part in the social affairs but I personally wasn't very much aware of their scientific programmes.
- VM: So, it was a bifurcated structure, was it?
- MT: I think it was. Or, shall we say, it appeared to me to be one.

- VM:** As a lab. worker, and not in any sense an administrator, you were conscious of there being another lot, whom you didn't have a great deal of contact with.
- MT:** Yes. I do remember at that stage, we are talking about diversification now from the photosynthesis as such, Al Bassham and Andy Benson were getting interested in photosynthesis in the oceans and the uptake of CO₂ in the air and the rate of photosynthesis. I think Al Bassham had been in the Navy, and had got naval contacts so work was, I think, starting to move...I don't know how far it got or what they did. But when you consider now the interest in that subject, the importance of the oceans in the CO₂ buffer situation, it's interesting that they were thinking on those lines at that stage.
- VM:** That was before '54, wasn't it?
- MT:** About '54, yes.
- VM:** Well, Al didn't do that but, of course, Andy did. Andy eventually ended up at the Scripps Institution of Oceanography at La Jolla and he has essentially been working in those sorts of areas now for many years.
- MT:** He must have moved back from the east coast to La Jolla.
- VM:** He did; when we saw him in the summer of '96 he said he had been there for 33 years.
- MT:** My impression is that Andy actually had a very innovative brain. I think that...I don't want to...subsequent (?) comparisons tend to be invidious...I think that Al Bassham was very much more a nuts and bolts person and day-to-day working and programming. I think Andy Benson had more vision and so that he took on the CO₂ in the oceans and so on doesn't surprise me. And, I think, because of that vision he probably made a very great contribution to the Calvin group.
- VM:** I think everybody who knew him in that context recognises that. You must be aware that for a long time it was often called the Calvin-Benson cycle or the Benson-Calvin cycle.
- MT:** I didn't realise that.
- VM:** There was a time when it was; some people used the term more than others. I'm not quite sure how it's referred to now because, in a sense it's old hat, and textbook material.
- MT:** I certainly had that feeling, that gut reaction, that Andy was a person with imagination and vision and, therefore, could make a great contribution to any programme he joined.
- VM:** I think that my experience (and I'm interested to hear yours) was that most of the people who were there were actually rather good, the people who worked in the lab. Was that your impression?
- MT:** Indeed, yes. I think that certainly I would say so. I think that if you looked at subsequent careers this has probably been borne out.

VM: What was your subsequent career? What happened when you left Berkeley?

MT: I resumed an ICI Fellowship in London. Jim Baddiley had left the Lister Institute and gone up to Newcastle, so I went to University College, the Chemistry Department. I was trying to resolve, optically, phosphate esters.

VM: Sorry: which esters?

MT: Phosphate esters; phosphorothio esters. In fact, I ran into great difficulties there and never achieved that. Subsequently I realised the things I got had such low optical activities that it would have been difficult for me to make the measurements reliably. I also got collaborating with Charles Vernon and his group on enzymes and so kept in contact up until his recent death with Charles. We had a great mutual interest in the phosphate-rich energy bond, the triple bond of ATP.

VM: He and Barbara...what was Barbara?

MT: Barbara Banks...Well now, that went back to a paper which Charles and Moore...George Moore — have you ever come across him?

VM: No, I don't think so.

MT: ...and Ron Gillespie, professor of chemistry out in Canada somewhere? They wrote showing that the concept of the energy-rich phosphate bond was a mistake, a misunderstanding in thermodynamics. The phosphate bond, any chemical bond, is an energy minimum, not a maximum. I read this when I was at the Lister Institute and as a chemist immediately saw the sense of this. I got in touch with Charles Vernon — used to attend some of their seminars at UC — and we realised we had a similarity in outlook. I became, I think I can say, notorious at the Lister Institute because I used to champion the fight against the energy-rich phosphate bond and Charles Vernon used to like to remind me I was the only person he ever knew who had been eliminated from a dance because they didn't believe in the energy-rich phosphate bond!

VM: Eliminated from a dance?

MT: Yes. The Christmas Party at the Lister Institute; one of the eliminations was anybody who does not believe in the energy-rich phosphate bond had to leave the floor. I was the only one. (*Laughter*)

VM: Oh, I see. it was an elimination dance!

MT: An elimination dance, yes.

VM: I see. Did they know that you were the only one?

MT: I told Charles this and he was much amused. But of course, Barbara Banks still has continued writing on this subject so this misconception still carries on. But anyway, I had these links with Charles Vernon and worked there for a couple of years; published some papers with him on enzyme chemistry. Then I left to go to what was then the Tropical Products Institute.

VM: In what capacity did you...?

- MT: I went as a section leader. I was in charge of a group working on the biosynthesis of pyrethrins. Of course, my work with C¹⁴ came in.
- VM: So this would have been in the late '50s by then, would it?
- MT: '57, yes. At that stage, I was in just charge of a small section working on the biosynthesis of the pyrethrins and their chemistry and discovered a new pyrethroid which was based on jasmone; chemistry as well — organic, classical chemistry. Then one began to get other responsibilities for pesticides in general. I became a member of WHO Committees on Pesticides, FAO and you gradually ascend the ladder and you get responsibilities for pulp and paper, and became deputy director and you have the lot.
- VM: Eventually, you were director...
- MT: Yes.
- VM: ...by which time it was called what?
- MT: Well, we went through various names. Tropical Products Institute for a period and then the Institute was amalgamated with the Anti-Locust Research Centre in South Kensington.
- VM: These were all, of course, government supported...
- MT: They were government supported by the Foreign and Commonwealth Office from funds devoted to aid. You clearly couldn't have a laboratory which had big entomological section on locust control called the "Tropical Products Institute". So it was called the "Tropical Products and (*indecipherable*) Natural Products Development Institute (TDRI).
- VM: One of these snappy civil service names!
- MT: One of these different names. It has gone through several names like that. Then we added to it a group of agricultural economists who were concerned with assessing land use management and we became the Overseas Development Natural Resource Institute (ODNRI). And I think it's something like that right now.
- VM: This has been funded under the Overseas Development Administration programme?
- MT: Yes, that's right.
- VM: As an aid activity?
- MT: As an aid activity. Now, of course, they are seeking to privatise. Subsequent to my leaving the Institute, it moved down to Chatham in Kent. One of my last jobs was gearing up for that move.
- VM: You eventually became the director of the whole organisation? How long were as director?
- MT: I think...let me see; I forget; I suppose about four years. I'm guessing four years.
- VM: And you retired in about '85?

MT: '85, yes.

VM: And live a life of luxury in Norwich since?

MT A happy life. Retirement can be very rewarding if you plan it and don't seek to do exactly the same things you've done in the past. I think it's a time for taking on something new, (*using*) new talent.

Tape turned over

SM: You mentioned your culture shock when you arrived, at seeing the Campus Police with guns in holsters. Can you tell us something about how you found living in Berkeley at the supermarket-domestic level as compared with what was very much like post-war England still at that time?

MT: There was a contrast, a quite definite contrast, between UK and Berkeley, apart from the weather, of course — everybody, I suppose, comments on that. The campus had got, at that time a lot of students in the “flower power”, so there was a lot of student activity, one was aware of the political feeling arising fairly strongly. The laboratory, whilst aware of it, was not part of it, not being at sort of student level. I think also the equality of it struck us on occasions. For instance, the swimming party. We went swimming there. You stripped naked and you had a shower and put on a grey sterilised costume. Rod Quayle and I were amused that you might be standing next to a student one time or a Nobel Prize winner the next, but you were all naked in the same shower. Now we felt this would not happen in UK, there would be segregation of the ranks. It’s like the officers and the men! So that was a distinct change.

Of course, San Francisco itself was an enormous shock because that's an entrepôt for things we had never seen in the UK. I first got interested in the differences in Chinese jade through the shops in San Francisco and, of course, the museum on the campus — there is some lovely jade in the campus museum. Also, I enjoyed access to the stacks (*in the library*), again being on the site, to be able to go down to the stacks in the library and, no matter what the subject, pick out books at your choice.

VM: At that time you couldn't do that in UK libraries?

MT: No, you put in...unless, of course, it was a reference library. But in Berkeley all the literature of the world was there on stacks down below and you were allowed to peruse it, I was allowed to peruse it and students were as well. There was a great freedom of access there which the system allowed.

VM: Did you find anything strange and unexpected when you got there that you can now remember? To prompt you, the example that I can think of in our own lives was when we once went into a post office to make a telephone call only to find, of course, that post offices were not the same as the telephone company as had been in Britain.

MT I think we were warned about certain sort of differences in terminology, so that didn't come as a shock. Something is flitting through the back of my mind which was a bit of a surprise but that's gone again. It was all probably the freedom, I think, and the interchange, perhaps, was a bit freer than in England although I had been fortunate in working in departments which were friendly.

- VM:** It was all very easy, of course, wasn't it, because the language was the same and you rapidly found out how everything was done and things worked.
- MT:** Slight differences in usage but nothing that caused great shock.
- SM:** How did your wife feel about it?
- MT:** She enjoyed it. Of course, she had travelled on the Continent before going out to America. As you say, it was easy with the language travelling there. We much enjoyed the camping and being able to get up into the Sierra.
- VM:** So it seems that you have pretty fond memories of your time?
- MT:** Yes. Filled with admiration for the individual Americans although I think sometimes the collective Americans...
- SM:** That's lovely!
- VM:** One last thing I remember, and I think, it may be something Rod said. Did Rod come and work in Tropical Products?
- MT:** Yes, that's right. You see, he came...
- VM:** Rod Quayle. I should say this is Rod Quayle for the record.
- MT:** He returned, you see, to England a bit before me, probably six months before me; I forget the actual time. He came back to a job in the Tropical Products Institute and when I came back he moved on to Oxford. And so the job he was doing became vacant and I applied for it and got it. I followed on directly from Rod Quayle.
- VM:** I See; but there was no...it wasn't the fact that he had been there that led you specifically to that place?
- MT:** No, no, it wasn't. In fact, I had worked — I shall have to backtrack now quite a long way — I was a student during the war, and as such we were under the auspices of the Joint Recruiting Board. When we graduated, the war had just ended — this was in 1945 — and so as students we saw the VE celebrations in Trafalgar Square and somewhere have got photographs of being hauled off by policemen from the tops of air raid shelters we were clambering on, so there was no need for us in the Armed Forces. So the Joint Recruiting Board could allocate you to a job in a factory, or in essential work, or in the forces. I went first of all to work with a consultant chemist...I got a job with a consultant chemist which was very wide experience. We did all sorts of analyses — for instance, greyhound doping in dog vomit, arsenic poisoning in elephants' intestines, some very amusing anecdotes from that period. But I got a knowledge of essential oils there. Again, under the auspices of the Joint Recruiting Board, I applied for a job at what was then the Imperial Institute in South Kensington. I worked there on essential oils. And then I was allowed back to become a full-time PhD student.
- So I had behind me from early days — I graduated at about 19 on one of these accelerated courses during the war — I had behind me analytical experience and a knowledge of essential oils. So I was able to fit into the Imperial Institute. Now, the Imperial Institute ultimately became the Tropical Products Institute. So, you see, I went back again.

VM: So you had contacts and you knew...

MT: I knew quite a lot about the work and the subject matter. By chance, Rod Quayle — it was purely accidental that we followed one another.

VM: Good. Well, I think we have explored your California era extensively. Shall we call it there?

MT: If anything occurs to me I can get in touch.

VM: Indeed. Well, it's been a great pleasure and thank you very much for your time.

Can we revert for just one moment to the deerstalker hats? You got them, did you?

MT: Yes, I ordered them through a shop, I think it was in Regent Street. I wrote off and we had to find out how to measure head size. I don't know if you have ever gone into measurements of head sizes but we did this and I think we got four or five.

VM: How did the deerstalker idea arise?

MT: I think it must have been Rod and I joking about Sherlock Holmes; I think this is how it arose. Then Calvin or somebody else, Clint (*Fuller*), asking what deerstalkers were. The situation built up finally to ordering them; I think it was from Lilywhites; Lilywhites I believe I got them.

VM: I think it might well have been. It's just such a famous picture.

MT: I can tell you because I've still got mine and I can look inside and see if there is a maker's name.

VM: Anyway, we look forward to getting a photograph of you in your deerstalker.

MT: You asked about cultural shock. Well, I can remember two instances in this. First of all, the hospitality of the people in the laboratory was immense and I think all of us enjoyed parties, outings and so on. But shortly after arriving there we were invited to a party —it might even have been a Christmas party — and our host, whose name I forget at the moment, threw open the door and said "Hi folks! Here's Nancy and Malcolm, Mal Thain from London". Now, I had never had that sort of introduction before, neither had I ever been called Mal Thain! This was a bit of a shock. But the other one, another Christmas party, we were chatting away, and the son of the host was sitting there. And I thought what an intelligent child, sitting there and lapping up the conversation, not asking any questions, but taking a great interest in everything I was saying. He finally revealed it all when he said "Gee, you do talk funny!".

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name ERIC MALCOLM THAIN

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Father's full name ARTHUR ROBERT THAIN

Occupation SURVEYOR Birthplace GORLESTON NORFOLK U.K.

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Your spouse NANCY GARDITT THAIN NÉE KEY

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Where did you grow up? BROMLEY KENT

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Areas of expertise CHEMISTRY, OVERSEAS DEVELOPMENT

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Other interests or activities NATURAL HISTORY, ENVIRONMENTAL SCIENCES

Organizations in which you are active Royal Chemistry Soc.

NORFOLK NATURALIST TRUST NORWICH, NORFOLK

NATURAL HISTORY Soc.

Chapter 38

DUNCAN F. SHAW (with Elizabeth Shaw)

London

November 25th, 1996

VM = Vivian Moses; DS = Duncan Shaw; ES = Elizabeth Shaw

VM: This conversation with Duncan and Elizabeth Shaw is taking place in London on November 25th, 1996.

Duncan, can we start by finding out from you what your pre-Calvin career was and how you came to be in Berkeley?

DS: Well, I started off going to Manchester as an undergraduate; that was from 1948-1951. I did my national service in the airforce for the next two years, then went back to Manchester as a research student in '53 and spent the next three years working with Professor Barker on carbohydrate chemistry.

VM: Was this Barker in Birmingham?

DS: No, Geoffrey Barker in Manchester.

VM: OK — because there is an Alan Barker in Birmingham.

DS: When I was in Manchester, Geoffrey Barker wasn't a professor even; he was elevated at a later stage.

VM: So you were a carbohydrate chemist during your research career?

DS: Yes. I was working a specific branch of carbohydrates, really; it was trying to work out the conformation of ribose in particular but carbohydrates in general, the way in which periodate oxidised the rings.

VM: That was your PhD period?

DS: Yes. So in '56 I finished. During the last year, Calvin visited Manchester and gave one of his talks. I was looking for something to do, like to go the States for a postdoc., and being involved with carbohydrate chemistry and since he had just given us a talk on the carbohydrate cycle, photosynthetic cycle, I was interested in that. What

fascinated me was that I thought that he had sorted it out the carbohydrate cycle and, as I was interested in physical-organic chemistry, I thought it would be interesting to look at the photographic — not the...the photochemical aspects of carbohydrates and how the light energy was tracked. I talked to Calvin, having been introduced by my supervisor...

VM: While he was here?

DS: ...while he was in Manchester during the course...on his tour through the lab. he'd stopped and had a word...and I told him what I would like to do. He said "OK, apply for various scholarships." He mentioned the NATO fellowship and the Commonwealth Fund fellowship. I applied for those and I got the Commonwealth Fund fellowship. I wrote to him saying I've got this, this is what I would like to do and would he take me into his lab. The letter came back "yes", so that's how I got there. We got married just before we went.

VM: Literally just before you went?

DS: A few days.

VM: How did you travel?

DS: On the *Mauritania*...

VM: And then across country?

DS: ...and in New York where we were met by the Commonwealth Fund Fellow people, topped up with money in the excellent way that they do, and then we got a train across country, first from New York to Buffalo, then Buffalo to Chicago and then the California Zephyr from Chicago to Oakland.

VM: This was essentially a non-stop journey or did you stop in these places?

DS: We stopped briefly in Buffalo for a couple of days to look at Niagara Falls and to meet some distant relatives of Elizabeth's who were living in Toronto. We didn't really stop in Chicago, it was just a change of trains. We spent one day in Salt Lake City.

VM: Do you remember your arrival in Oakland? Were you met?

DS: We were met by the senior lab. technician...

VM: Paul Hayes.

DS: Paul Hayes; he met us off the train and took us to Calvin's house where we were looked after for two or three days until we got ourselves an apartment.

VM: You, of course, had already met Calvin in Manchester.

DS: Very briefly, yes.

VM: How did he strike you as a personality when you first...well, you would have first seen him at the lecture, would you?

DS: I was impressed by the lecture, of course. I didn't form much of an impression during the brief word I had with him. He was friendly enough. He was certainly very friendly when we first got there and looked after us well. Took us up in his car for the weekend in the hills with his children and was most helpful.

VM: And then came the day when you went into the lab. This was presumably not the day you arrived.

DS: No, perhaps the day after; probably the day after.

VM: Can you remember what went on, then, when you first went there?

DS: I went into his office certainly and what happened there was, perhaps, a bit disappointing, looking back on it as far, as I was concerned, disappointing for myself more than anything in that he sort of said "OK, you came here to work on the photochemical aspects but we've got this problem and your background in carbohydrate chemistry and periodate oxidations would be very suitable to solve it. Would you like to do that instead?" I suppose I was interested but it did, in fact, turn out to be a bit of a red herring.

VM: What was the problem that he wanted you to work on?

DS: The problem was that Ozzie, Osmund Holm-Hansen had done an experiment, which was alleged to be done in the dark, and he'd found radioactive PGA and this was surprising. The idea was that I would use my carbohydrate chemistry to chop up the PGA and find out where it was radioactive. Having started doing that, I then started to break it down. I had a longer talk with Ozzie to find out just what the background for this experiment was. I have to say I was rather disappointed in that it had been done once; there had been no controls and I wasn't convinced that the thing was really done in the light when I heard how he had done it. For example, he held it up to the light, to the dark light in the darkroom and poured the alcohol in to stop the reaction. How do you know that light couldn't get in at that stage? He just assured me that, oh no, it wasn't bright enough.

I repeated the experiment, taking rather elaborate steps if you remember...I don't know whether you do: I had a bottle on my bench wrapped around with black insulating tape, so that everything could be done in the dark. I injected the alcohol into, hot alcohol, into it in the dark so there was no doubt there was no light in it. The amount of radioactivity I got fixed was so small, it was almost negligible. I did actually try to find, you know, analyse it and find the radioactive materials in it but the levels were so low that the chromatogram showed virtually nothing.

VM: How long did you spend doing that?

DS: I suppose it was about three or four months.

VM: So you learned all the techniques in that time.

DS: Yes, I used the chromatography, and the breakdown of the radioactivity and the counting equipment and so on. What I was disappointed more than anything was that instead of doing what I feel I should have done — this is what I came to do, this is what I'm interested in — and, of course, that would have been from a career point of view it would have been getting into photochemistry at an appropriate time. I ended

up falling off a red herring, I think! I can understand Calvin wanting this done, so I'm not blaming him at all.

VM: But that was for four months.

DS: That was for about four months.

VM: And then what?

DS: Then there was another problem cropped up which was one, I think, that Otto Kandler brought in where he questioned whether...No; he had done some cyanide poisoning experiments. Instead of stopping the reaction with alcohol, hot alcohol, he stopped it with cyanide and found that he got a different chemical. This might have been one of the key precursors of constituents in the cycle. The idea was that I would work on that, to find out whether that was the case. In fact, it was Bob Rabin that came up with the idea that this material, hamamelonic acid, was the one, the chemical...it was suggested was important. Bob Rabin sort of came up and said that well, this had been made, it has already been found by a chap in Germany who's got a sample; let's send for some. Calvin did that. In the meantime, I did the experiment with...was it Joan Anderson?

VM: Jan Anderson.

DS: Jan Anderson, working with her, where we did the cyanide poisoning experiment but used radioactive C^{14} -cyanide. We did actually fish out some hamamelonic acid which we identified through using the sample from Germany. In fact, the radioactivity was from the cyanide and not from the CO_2 , so effectively knocking Otto Kandler's suggestion on the head. So effectively I spent my time sort of following up two red herrings. It was necessary to follow up but they weren't quite the exciting thing that I thought I was going to get involved with.

VM: Did you not find in the context of that lab. where everybody, it seems to me looking back on it, got involved in everything that they wanted to get involved with, that you couldn't have done photochemical work as well?

DS: Not really, I don't think, because, as I found afterwards, there wasn't any real photochemical work going on. Calvin actually, surprisingly, didn't have a great interest in that end of it, as far as I could tell. Who was the Japanese?

VM: Power Sogo.

DS: Power Sogo was working with electron spin resonance. I think he was the only one who was working on aspects that might have been related to photochemistry. He was working in a different...he had his equipment somewhere else. Although I knew Power Sogo was around, he was the only one that was working on that aspect. So, logically, if I had struck out and said "Yes, I wanted to work on the photochemical bit", I would have ended up working with Power Sogo, which might actually have been more along the lines that I was interested in, although I don't know that it was ever a major interest of Calvin's.

VM: When you had originally talked to him in Manchester and said you wanted to work in the physical energy area, he'd gone along with that?

- DS:** Well, I don't know; it's a bit difficult to say. The conversation I had with him at that stage was a very brief one. We certainly didn't discuss technically what the options were and what the possibilities were. I think the problem was just mentioned...looked interesting...I would like to...
- VM:** Did you get any publications out of the work you did there?
- DS:** The work with Jan Anderson was published.
- VM:** You had a bench, presumably, in ORL, did you, in the big room? I don't remember where you were.
- DS:** I was within a few feet of the hole in the floor where the original cyclotron sat.
- VM:** Do you remember who your neighbours were in the room?
- DS:** I think that Ning Pon was behind me. Immediately behind my bench was a big rack for attaching apparatus to, and I think he was on the other side of that rack was my recollection. Across the bench in front of me was Karl...
- VM:** Lonberg.
- DS:** ...Lonberg. I think that was the layout in my bit of the lab. I think they were the only ones that actually worked with benches in that part of the lab. although there were other things going on, there was a big UV machine, recording UV machine, in the same room which a variety of people used. There was a big table in the middle around which people gathered for coffee.
- VM:** You were in that room with the big table in the middle?
- DS:** Yes.
- VM:** My own memory is that there were more benches in it than just three but I've got a vague memory. Later we can look at some pictures which may resolve that particular point. How did the style of working in that lab. strike you compared with where you'd come from in Manchester? Different? Same?
- DS:** I think there were many similarities. People were milling around talking and you could discuss almost anything with anybody so you could find out what was going on, what other people were doing. Many of them were using common techniques and so there was a lot of exchange of information on the techniques and, in particular, I was interested in Helmut Simon's experience of measuring radioactivity. The standard method in Calvin's lab. was do a chromatogram and then put a counter on top of the spot and count it which was a quick and effective way, but a fairly crude way of measuring. Whereas Helmut Simon had a very sophisticated way where he converted the chemical into CO₂ and counted it in a very much more accurate way. I went over with him once and watched him and learned a little bit about his counting techniques. So that was a good exchange of techniques and so on.
- VM:** Had there been that sort of thing in Manchester?
- DS:** Oh yes, I think so.
- VM:** So this whole style of doing things was not unfamiliar to you?

- DS:** No, it was quite similar. People talk about their chemistry and what they were doing over coffee and swap ideas.
- VM:** What about the lavishness of the support in Calvin's lab.? Was that a novelty?
- DS:** Not...I think he had quite sophisticated counting equipment which seemed to become available as soon as it was wanted but since I hadn't been doing any radioactive work in Manchester, and I don't think anybody else was at the time, we wouldn't be using that sort of equipment anyway. But Manchester was quite well equipped. I think there certainly was more money available there (*in Berkeley*) than we had had at Manchester and there was some quite sophisticated counting equipment became available. But then at Manchester I never felt the work I was doing was held up for lack of money, but perhaps the sort of thing I was doing didn't require large amounts of money anyway so I wouldn't have come across the problem.
- VM:** What sort of relationship did you think you had with Calvin as, after all, the leader of the group? What was *your* relationship? How did you seem him functioning in the group as a whole?
- DS:** I think on personal relations I found him...he was easy going and friendly, no problems at all.
- VM:** Accessible?
- DS:** Yes. He took an interest and he would like to know how you were getting on, particularly the Jan Anderson experiments because, in a sense, his ideas were being challenged by the suggestion that the cyanide poisoning experiments had shown up this new compound so he was rather interested and, I think, happy when we found out that the hamamelonic acid came from the radioactive cyanide and not from the photosynthetic cycle. So he was interested in that. We devised a little modified bit of apparatus to enable us to stop the experiment very quickly in a way which the original set-up hadn't made possible. He was interested in that.
- VM:** How did you see his role in running the lab. or in being the inspirer of the work? Do you think he was or do you think...overwhelmingly?
- DS:** It's a little bit difficult to say there because I was coming to the lab. after the key work had been done. I imagine that he was a key inspirer of the original experiments and the work that was done. As I say, by the time we got there, working on the photosynthetic cycle, the bulk of the work had been done, we were tying up loose ends. I think there was probably not so much a need for inspiration at that stage because that happened. The work done was defending the idea which was done successfully in the case of the cyanide experiment.
- VM:** Did you get a strong feeling of "end of the project" while you were there?
- DS:** Yes, I think I did. I got the impression that he'd done the work, it had been accepted, it looked to be soundly supported, it stood up to the various attacks that were made on it, and there was not a great deal more to do along that line, I felt, which really was the feeling I had originally when I had heard Calvin's lecture. He'd more or less done it, I am sure I was wrong — there were loose ends to tie up, but the impression I got was he had more or less done it and that's why I felt it would be nice to go on to look at the photochemical aspects.

VM: Did it strike you when you were there that there was no great sense of where we go next in the group as a whole?

DS: I didn't detect that as a feel from the group as a whole. I think I felt that, that there wasn't a lot more that I would want to get involved with in that aspect of the work. Which is really why, when it came to having the option of continuing for a second year, I decided not to do that. I felt that I wasn't doing anything very exciting in continuing with that although the exciting work had been done before. It must have been really quite exciting, I think, during the early stages when they first found the PGA on the chromatograms and were chasing up the reaction pathways and developing the ideas. That must have been the exciting bit.

VM: From a social point of view, what do you remember about life in that group? Were you very much involved with other people in the group or did you make your social life outside it?

DS: A mixture. We had good social contacts with Ozzie and Harriet (*Holm-Hansen*), we went off on weekends on two or three occasions with the group and had a good time. Helmut Simon, I think, was the other person we made quite good friends with. But, of course, there were other people that I knew from Manchester. Ian Bell, for example, who I'd been a PhD student with at Manchester, was working with Rapoport. We went camping with them and on expeditions with them as well. So we had a mixed social life, partly with ex-Manchester people. There was a Manchester person working at Stanford, we met; there were two other people working down in Los Angeles so at New Year we all got together. Another one in Vancouver. So there were lots of Manchester people on the West Coast.

VM: When you say "Manchester": Manchester chemistry?

DS: Manchester chemists, yes. So we had contacts with them but also with the people from the group.

VM: What was the social life in the lab. like compared to what you had been used to in Manchester? Was that also a vigorous social atmosphere there?

DS: I think there were...it was similar, I think. There were some differences, of course. In Manchester, most of the work postgraduates, most were not married and followed the sort of pursuits that those people do. By the time we got to Berkeley, I got married, most of the people we were working with were likewise married, so you had a different sort of social life with that different situation.

VM: But the postdocs. for the most part, maybe entirely, had no children at that stage?

DS: That's probably right.

VM: Some of the permanent people by then would have had, I suppose. So, we were free domestically in that sense...

DS: Yes.

VM: ...enabled us to run around a lot. Sitting next to me is Elizabeth. What was it like for you coming to Berkeley and being, as it were, on the periphery of the lab. but not actually spending all your days there?

- ES:** Well, I thought it was terrific when I first got there because I had no concept of what it would be like at all. I was just amazed, partly at the weather, I think, because we used to...You were talking about the social side and my recollection is that almost every weekend we used to go somewhere exciting. We went skiing in the winter and could come back to this lovely warm west coast afterwards.
- DS:** We had some good beach parties. Who was the red-headed Swiss man?
- VM:** Utz (*Ulrich*) Blass
- DS:** Utz Blass; we had...I remember a beach party with Ozzie and he was there, a picnic on the beach with bottles of California wine, up north, somewhere near Drake's Bay, I think.
- VM:** Stinson Beach. I think I have a picture.
- ES:** I remember saying, "Oh, we'll go swimming" and being quite sure that, coming from Lincolnshire where the water is pretty cold, I would have no difficulty in swimming off San Francisco. But, of course, I was rudely awakened. I don't think I ever got in.
- DS:** You were put off, I think by people who told you that the current is too cold...
- ES:** ... and it was dangerous; I don't know whether it really was. So I never swam in the sea.
- VM:** Did you work while you were there?
- ES:** Yes. I worked. I had a bit of a difficulty in getting a job in the first instance but in the end I got one and I worked full-time.
- VM:** What was it like for you domestically, the American shopping scene and all those sorts of things must have been different from what you'd been used to?
- ES:** Mrs. Calvin took me shopping and I was going to buy about two eggs and things like this because I was newly married and had no idea about how to buy the right amount of things. She was ever so sweet and said "you know, really, I do think you'll get through six eggs". She sort of initiated me...was it the Co-op?
- DS:** the Berkeley Co-op, yes. Yes, I'm sure it was; she took you to the Berkeley Co-op, the first of the supermarkets...
- ES:** I had no experience with supermarkets at all
- DS:** ...where you trundled your trolley full of goodies out to the car park. Something like that we didn't have in Britain at that time.
- VM:** I remember also that you acquired rather a splendid car. What I remember particularly about that car is that it had a fluid flywheel. You changed gear but you had no clutch. Am I right?
- DS:** That's right, yes. It was a semiautomatic; it had the fluid flywheel and it had a gear lever. You only needed to change...you had a range from first to second, which you would change between by lifting your foot off the accelerator and it changed up

automatically. You then had to change manually from second to third and then the same automatic process worked from third to fourth when you lifted your foot off the accelerator. But in normal driving, once you were in third and fourth and you stopped at traffic lights, you didn't bother to change down because the fluid flywheel and the big engine gave it enough flexibility so you drove around without having to use the gear lever most of the time.

ES: Ah, but the biggest asset of that car, from Duncan's point of view, was that it had a "whooper"...

VM: It had a what?

ES: A "whooper"...

DS: Almost like a whistle.

ES: ...so if he saw some really gorgeous female he could...

DS: Now, now. I didn't go in for that sort of thing. The only thing I used to use it for was if I arrived back from the lab., or wherever I had been in the car — not that I was ever allowed to use the car once she'd learned to drive — I could make this whistle work and she would know I had arrived. It actually was illegal to use it in California.

ES: It had belonged to a student, hadn't it?

DS: Yes. Well, it was the son of the lady we bought it from had fixed this whistle. And you had a little string down here you pulled and it whistled. It was meant for the sort of activity that Elizabeth mentioned. Naturally, I didn't use it for that purpose!

VM: Where did you live?

DS: We lived in Hilgard Avenue which was five minutes walk from the campus. Absolutely perfect, couldn't have been better situated from the point of view of walking to work. Before Elizabeth starting working, I walked back for lunch.

VM: Where did you work when you started?

ES: Richmond.

VM: So you had to drive.

ES: I had to drive. That's why I had the car every day. But he didn't need it anyway.

DS: I did have to teach Elizabeth to drive.

ES: Yes, he did; he did teach me.

VM: So you could drive beforehand?

DS: I had the English licence, yes.

VM: You were in the RAF Volunteer Reserve at the time, weren't you?

DS: I was in the...actually, I had been in the RAF for my national service but I switched into the Naval, Fleet Air Arm Reserve. I had been in that for the three years as a postgraduate student.

VM: There's something I remember — but it's probably pretty distorted now because the memory is forty years old — had you had an aerodynamic accident of some sort?

DS: That's one way of putting it, yes.

VM: I remember we teased you and said you would have to pay for it at the rate of sixpence a week from your salary. What happened?

DS: I had been involved in a mid-air collision in a Vampire...

VM: Oh, really/

DS: ...and jumped out.

VM: Successfully!

DS: Successfully, yes.

VM: Do you only do that once. What does it do to your confidence about flying when that happens?

DS: Well, I suppose it makes you a little bit more wary but I was half-way through a jet conversion course and went on and finished the course. That was early on in the airforce and I went on to do a flying instructor's course.

VM: It didn't turn you off flying.

DS: No. And then I continued through jets again in the reserves and was doing so right up to a couple of weeks before going to California.

VM: When you came back to England after that, did you take it up again?

DS: Whilst I was in California I was trying to get attached to one of the American naval reserve squadrons which would have been possible although the regulations wouldn't have allowed me to fly an aeroplane myself;. I could have gone up with other people. But, in fact, part way through these negotiations, the UK government abolished the auxiliary airforce and the naval reserve squadrons. So I got a message from the people in...I think it was the Naval Attaché in Washington, saying "sorry, forget it all" There's no more reserve flying for you when you get back so there's no point continuing as an attachment to an American squadron".

VM: They terminated your association with the reserve just like that?

DS: When I came back, the squadron had been disbanded. I stayed on the naval reserve list for some time...

VM: But not flying?

DS: ...there was no flying.

VM: You said earlier on that one of the reasons you didn't want to stay on in Berkeley for a second year was that you felt your research wasn't going in the direction you wanted it to go. What, then, did you do?

DS: I came back to this country and took up a job that had been offered by ICI at Billingham.

VM: That job had been offered before you went to Berkeley? It was waiting for you?

DS: Yes.

VM: What sort of work did you do there?

DS: It was general work on hydrocarbon chemistry, a variety of things concerned really with making use of the materials that come out of cracking hydrocarbons, ethylene, propylene, the sort of chemical reactions that those undergo and can be converted into more useful things that sell for money.

VM: Were you thinking of making your whole career then in an industrial context?

DS: At that time, yes, I thought it would be worth a try. But after a couple of years I decided that the chemistry wasn't going very far.

VM: Your chemistry or their chemistry, or chemistry in general?

DS: I didn't find it very interesting, the chemistry I was doing, I suppose.

VM: So what happened next?

DS: I went to the University of Liverpool into the organic chemistry department under Professor Kenner...

VM: As a lecturer.

DS: ...initially as a senior research assistant, I think I was called — it was a permanent post as opposed to a postdoc. I had a number of different responsibilities. Instead of going to do research and teaching, which is the normal mix, I went there to do research and running the lab. so it was an administrative-cum-research job. I had research students and gave some lectures at the honours level. But I had responsibility for seeing that the lab. ran properly. That was quite a heavy load at the time because they were just getting completely new organic chemistry labs. built from scratch. I was heavily involved with the equipping, furnishing, setting up and running the of the labs. for the first three...four years. After the fifth year, things had settled down, and I was getting on with doing more research.

VM: If I put some dates into this: you were at ICI roughly from '57 to about '60?

DS: '59.

VM: In that case, to about '61 or so?

DS: No: I went to ICI in October '57 and stayed there for two years. I left in October '59.

VM: That's when you went to Liverpool?

DS: Yes.

VM: OK. You've taken us through to about five years into your Liverpool stay, by which time everything had been organised and you were in the clear, as you were saying.

DS: Yes — more or less.

VM: So that's '64,'65-ish. You continued doing that sort of work?

DS: Then I was offered the post of Sub-Dean of the Science Faculty which involved a promotion to the senior lecturer level. So, I moved into that post. Again, it was running the Science Faculty; that was more administrative and rather less academic.

VM: Was it taking you away from research and teaching?

DS: Yes; the agreement was that I could continue to do research in chemistry which I did to a certain extent and wrote a couple of papers whilst I was there. But then I got involved with applying computers to the administration student records, and so on. After about four years of that, the idea...well, it was actually suggested that I should apply for the post of Academic Sub-Dean, Academic Secretary, in the university — a level under the Registrar. When I broached the subject with the Dean to see what did he think of this idea, he said "well, did you not know that you might be made full-time dean sometime?" Which I said that I didn't. The Science Faculty started then under his initiative — he was a professor of chemistry so we had a lot in common anyway. He got the university, the Faculty of Science to agree to having a post of full-time dean. The university didn't like that idea. They had just had a full-time dean of medicine go to sleep in the job.

VM: Hadn't deans been elected before then?

DS: They were not elected; they emerged...for three years.

VM: But they were proposing that you would be a permanent one?

DS: That was the idea, yes. They didn't like the idea of the Science Faculty doing this experiment. So, they compromised and made me a post of Pro-Dean. Essentially I did the same work as I would have been doing, had I been permanent dean, but there was a part-time dean to peer over my shoulder.

VM: And you did that for a long time, didn't you?

DS: For a long time, yes.

VM: Until formally you retired?

DS: Yes, although I got involved with other things. I started the Centre for Marine and Coastal Studies. I had been made Director of that and had been doing that on a part-time basis. When I retired as pro-dean, I kept on the part-time activity of being the Director of the Centre for Marine and Coastal Studies, which is where my interest in nature conservation and marine conservation came from.

VM: What contact have you had with Calvin's lab. since you left?

DS: None, really. I got a letter, I think, just after he got his Nobel Prize. I had contact...I was written to and asked if I would send a comment on the bottom of a publication, which I did but I haven't had any contact since.

VM: Never been back?

DS: No, we've never been to North America. Oh, we went to Newfoundland for a week. Haven't been to North America since.

VM: Have you kept up with any of the people that you knew there at the time?

DS: Only Ian Bell, who was from Manchester anyway, who we were friendly with while we were out there; we've kept up with them. But I haven't kept up with any of the others; that's right, isn't it?

ES: Not from Berkeley, no.

DS: No. Not from Berkeley, no.

VM: When I saw you in Liverpool in about '81, I think it was, I'd had no contact with you for about twenty years. I knew that you had gone to Billingham and then — I don't remember how I found out that you had gone to Liverpool but somebody must have said or something — and I was going there for a meeting, I got in touch. Looking back on your own Calvin period, what did it do for you?

DS: I think socially and the opportunity to live in another country, particularly California, was fantastic. It was interesting to see the facilities and the way they worked in Calvin's lab. All that was positive. The chemistry, the actual chemistry I did was not as positive as I would have hoped.

VM: Careerwise, was it an insignificant interlude?

DS: I think it was a disappointment from a purely scientific point of view; looking back on how it possibly could have affected my future career, it was a bit of a disappointment.

VM: You don't think it influenced your later career?

DS: No, I don't think so. Whereas it could have had a positive influence. So, I think it was fairly neutral. Nice to go there; I'm sure I haven't lost anything by having gone and I'm glad I went.

VM: A lot of people have spoken about the uniqueness of the group being associated with a particular architecture of the building, the fact that it had these large, open labs. and people weren't hidden behind walls and doors. Did it ever strike you like that?

DS: Not really, because the situation in Manchester was not unlike that. When I first went to there (*to Manchester*), for my first year and a half, I suppose, as a postgraduate student, I was in a very large laboratory which must have had sixteen people working in it, one of these big almost like a teaching laboratory, benches with two people working side by side and two people opposite, and then another bench, and another bench. So there must have been at least sixteen people, maybe twenty people working there, all postgraduates so that there was a thorough mix-up of people. After that I then moved a little bit down the corridor into a smaller lab. with, I think, there were four people in that one. But again, we got tended to get together for coffee and...I

didn't notice any particular difference working with Calvin because I had never worked in a small lab. on my own or with just one other person. From that point of view the Calvin set-up was very similar to what I had experienced in Manchester. And, very similar to what we set up at Liverpool while I was there.

VM: You remember, of course, seminars...the Friday morning seminars.

DS: Yes.

VM: With pleasure, or not?

DS: Yes, I think so.

VM: They were very early in the morning.

DS: They were, yes. I don't think I went to all of them.

VM: Also, I have one particular memory which involves you. You remember there used to be a beer session on Friday afternoons, very often?

DS: Not particularly.

VM: There was. There was a place called Laval's: you do not remember that?

DS: No.

VM: ...where we would go out for beer? You and I were preparing to go to beer one such Friday afternoon, and we were standing in front of a blackboard, actually above the place where they dried the photograph films, I think: some sort of oven. We were doodling on the blackboard and we produced what we called the *Dephlogisticated Soot Cycle*? Do you not remember that?

DS: No, I can't remember that.

VM: Well, we started out off carbon dioxide polymerase to make all polycarbon dioxide and then all sorts of magic things happened. We were playing around with this and Calvin came through the door. We were suitably embarrassed and grabbed a duster and to start rubbing it out but he said "hold it, hold it. There might be something in it!" We then had to wriggle our way out of it.

DS: No; I've forgotten it.

VM: I am sure it was you because it was the sort of thing the British got up to in places like that..

Are there any other things that we've forgotten to talk about which you would like to bring up? What I'd like to do is show you some pictures and maybe after you've seen them you may have other things to say.

DS: That would be nice to see some pictures.

VM: OK. Shall we close it down now...?

DS: OK.

VM: Thank you.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Duncan Frederic Shaw
Date of birth 17th March 1931 Birthplace Grimsby Lancashire UK
Father's full name Duncan Blackstone Shaw
Occupation Storekeeper Birthplace Lancashire UK
Mother's full name Ada Shaw (née Roberts)
Occupation Housewife Birthplace Burnley Lancashire UK
Your spouse Elizabeth Margaret Shaw (née Wraithall)
Occupation Physiotherapist Birthplace Boston Lincolnshire UK
Your children Grant, Graham, Julia

Where did you grow up? Shoreham Sussex

Present community Hewall, Wirral

Education Worthing High School, Manchester University

Occupation(s) Chemist, University Administrator, p/t Nature Conservation

Areas of expertise Chemistry, Marine Sciences, Nature Conservation

Other interests or activities Sailing, Flying (Private Pilot's License)

Organizations in which you are active University of Liverpool, University College Chester, English Nature, Joint Nature Conservation Committee

Chapter 39

H. MONTAGUE (MONTY) FREY

London

January 6th, 1997

VM = Vivian Moses; MF = Monty Frey; SM = Sheila Moses

VM: This is a conversation with Monty Frey in London on January the 6th, 1997.

Can I start off by asking you how it happened in the first place that you got to Melvin's lab.?

MF: I think it was one of those real accidents. It wasn't absolutely clear that I even was going to be a scientist. We had a family business and I was really expected to go into that. I suspect I kept delaying so that when I had finished my first degree, I had been interviewed for a job with ICI and my father was fairly anti- and said "why did I want a job with ICI?" I said "well, because I'll then be able to afford a car".

VM: You had a chemistry degree?

MF: I was at Oxford and I had a degree. I was, as it happens, an able student. I didn't find it very difficult. And he said, "well, all right, I think you are being very silly, I'll buy you a car and you can do a doctorate. It will look good on the business letterhead, we'll have at least one director who is a doctor." So, I got the car and I stayed in Oxford and I did a D.Phil. with Hinshelwood. Towards the end of that period, I got engaged. The final year my wife-to-be lived in London and I was in Oxford, not unconventional in those days to have a year's engagement. I suspect that I decided to postdoc. for a whole range of reasons, none of which would be thought to be very valid nowadays. One was, I thought it would delay making the decision about going into the family business.

VM: What sort of business was it?

MF: Import/export.

VM: Nothing to do with chemistry?

MF: Nothing whatever. Secondly, I thought it would be quite fun to go to America and, thirdly, I thought it would be good to start married life well away from both sets of parents, especially my parents-in-law. That's only because my wife, you know, had been a very well-cosseted daughter and all the rest. So I discussed with Hinshelwood

where I should go. With Hinshelwood I did gas kinetics. At that stage I think everybody in the lab. thought I was a kind of dilettante scientist so it didn't really matter too much where I went or what I did. Really, the problem was just one of getting grants. In the end, I had three grants: one would have taken me to Washington, DC, one would have taken me to CalTech with Pauling and I then got a Commonwealth Fund Fellowship (they are now called Harkness Fellowships) and that really was, I think to some extent, I had to decide when applying for that where I would go. Hinshelwood, I think, suggested that Calvin would be possible.

I have to say in all honesty: I think I thought that Berkeley would be a nice place to go and who could I go and work with?

VM: You were not the only one to...

MF: Yes, well, as I say, none of this is academically sound, in a way, certainly not scientifically sound. The other thing about the Commonwealth Fellowship was it was easily the best in terms of the money and the fact that it paid for months of travel and it would pay something for a wife. Altogether, it looked the best bet. I think probably scientifically the best thing would have been to have gone to CalTech and work with Pauling. But — that was it.

VM: What did you know about Calvin and his lab. before you went there?

MF: Absolutely nothing.

VM: You had no idea what they did or what...?

MF: When I say absolutely nothing, I knew nothing about it when I chose it. I knew a little more about it when I decided that I would go there. I looked him up and I knew this, that and the other; and one of the reasons that there was a possible niche, though it didn't actually turn out that way, was that I had been using carbon-14, for totally different things, but I had been using carbon-14 as a tracer and he was very heavily into labelling things with C¹⁴ for photosynthesis. I think that was a sort of bridge.

VM: You hadn't met him?

MF: No.

VM: Had you met anybody who had been in the lab. before you went out?

MF: No.

VM: So you really didn't know...?

MF: Nothing, absolutely nothing.

VM: Had you agreed with him what you were going to work on?

MF: No, and he wasn't even there when I arrived.

VM: So what happened when you arrived?

MF: It was really quite extraordinary because Calvin was in Europe for three months.

VM: This was when; this was '54, was it?

MF: '55.

VM: '55?

MF: 1955; I got there in September.

VM: This must have been just at the time I was hearing him lecture here (*in London*).

MF: Yes, exactly; that's why he wasn't there! When I sort of think nowadays of people coming to work with me and all the rest, I'm amazed how little disconcerted I was when I arrived. Part of the reason that it was so nice and easy is that we had absolutely fabulous accommodation which no postdoc. had any right to expect and no postdoc. ever had. In fact, the guy from the Commonwealth Fund, who came out to visit us, a chap called Lance Hammond, said it was the best accommodation he had ever seen a Fellow in.

MF: What was it and how did you find it?

MF: Well, it was just luck. Typically, at this stage, I think I was already relying on my wife-to-be to do all the important things. And I think she wrote to the accommodation office in Berkeley and our letter arrived, as I understand it, as the wife of the head of the Classics Department popped in to see who they would rent their duplex to while he spent a year in Europe. I think somehow or other they must have got a feeling that we were much older and senior and reliable. So, we had this fabulous duplex within walking distance.

VM: Do you remember where it was?

MF: Yes, I think it was 2554 Virginia Street — it was certainly Virginia and it's the number that I'm not 100% sure of., I think 2554; just next to Euclid, on the corner of Euclid and Virginia. In fact, when I applied for a car parking permit, which they pointed out (a) I wouldn't ever get on the points that I could amount, amass; that that where I lived was considered a prime parking spot to the university and that if I didn't get my car out of the garage and park it outside the house, I wouldn't be able to park it outside the house all day because someone else would be parked there. So, it was really superb and he was Professor of Latin. My wife subsequently met his wife but that's another story. There were thousands of books in the house...

VM: ...all in Latin!

MF: No, all in English, I'm not quite sure why. Not only were there academic books but there was a huge range of novels. It really was great for anybody who likes books.

VM: This had already been set up so when you arrived you went straight into it?

MF: Yes. It was all set up at a distance, at a very low rent so that we should look after the place. It had a gardener with it, who came once a week. I think those sorts of things made it much less disconcerting that we had arrived and the guy I was supposed to be working with wasn't there.

I think I had discussions — I'm trying to think who I had discussions with; maybe it was Bert Tolbert or maybe it was Lemmon. But anyhow, I decided what I was going to do which was a sort of something with carbon-14.

VM: It was pretty much up to you to decide?

MF: Entirely at that point, since Calvin wasn't there. Indeed, for the first four months I worked by myself on that. I built my own apparatus, it wasn't too much different from what I had been doing in Oxford so it was relatively easy to do.

VM: What was the climate, the local...the laboratory climate in which you were working similar to the one you'd already met in Oxford or were there quantitative/qualitative differences in the kind of atmosphere, relationships, etc.?

MF: It's hard to tell at this distance. Most of my experience was, I suspect, a little atypical. In Oxford, Hinshelwood, after all, at that point was President of The Royal Society and a very powerful man, and head of the department, and the people who worked for him, I suspect, had charmed lives in terms of things being done for them. Like, I needed some glassblowing done and it was done within a few days. I only realised a good long time later that people waited weeks and weeks for stuff to get done. Nobody ever talked to me about things like budgets. So I think I had a very cushy existence in Oxford and it wasn't a lot different when I got to Berkeley.

Now, I suspect that if I had been through a normal course of things, Berkeley would have looked like a kind of scientific paradise in that lots of things were available. I remember going to the chemical stores that they had and there were like everything in the Kodak catalogue was there, somewhere. You could just pick it up and take it. Only when I got back to England did I realise how extraordinary this was.

VM: In Oxford, had you been working in a little room by yourself or a big room with others?

MF: No, I had a room which was about...nearly twice the area of this room...

VM: This room, for the record, is about 300 sq. ft..

MF: Yes. OK. It would be probably 450 I should think. When I started, I was the only person in the room and then a postdoc. arrived subsequently and the two of us had it. So it was very spacious.

VM: But, nevertheless, it was just you and one other?

MF: Yes.

VM: What happened in Berkeley?

MF: In Berkeley I was given a room which was much smaller but still of adequate size that I had it all to myself.

VM: And you worked in that way, all by yourself in a room throughout the period you were there?

MF: No, that was for the first four months before I then switched; I mean, when I switched and worked on NMR, the instrument was elsewhere but I still kept the room as an

office now and did no lab. work in it. I always was in a very happy state from that point of view.

VM: Coming back to the beginning, what did you start working on?

MF: I was looking for an isotope effect on the pyrolysis of pyruvic acid.

VM: As a pure chemical problem?

MF: Yes.

VM: Was this related to other things they were doing?

MF: I think they had got interested in isotope effects because they are quite important in terms of mechanism because you get fractionation and you may get very misleading things. That was one of the reasons why I thought I would do an isotope effect, it would fit in. I think Dick Lemmon had...I think it was he who had pyrolyzed pyruvic acid. They had been trying to make a labelled compound, I forget now exactly why. It was according to what was in the literature, the pyrolysis was totally atypical in that it led to the elimination of carbon monoxide. Now there are a large number of isotope effects involving the elimination of carbon dioxide and this would have been one of the first. if not the first. That's what attracted me. It doesn't pyrolyse homogeneously; it pyrolyses on glass surfaces. The sad thing at the end of the day is that Dick got it wrong. It actually gives off CO₂, so it isn't terribly interesting. The literature was wrong and, although I put it right. it became a very uninteresting study though it had some fun.

VM: That was what you were doing in the first few months?

MF: Mm.

VM: And that was, as it were, a separate piece of work; it was not part of some great team effort?

MF: No. None of my work was ever part of a team. So in that sense I was quite isolated. I was doubly isolated in a sense that I was very much a physical chemist, indeed a gas kineticist, whereas most of the group, of course, were not. They were much more knowledgeable in areas where I knew very little.

VM: What sort of interaction did you have with the people in Donner, members of the Calvin group based in Donner — and I don't know: there were ten or twelve of them there at the time?

MF: It was more social than scientific though at coffee times inevitably things were discussed. It seemed to me a lot of discussion was on technical things to do with apparatus functioning or equipment and how it functioned and not...well, not in my presence. anyhow...so much on scientific problems as I would see a scientific problem.

VM: In the absence of Calvin in Europe for those three months, was there much in the way of scientific leadership as you saw it?

MF: Clearly the Donner group was one group and the ORL was another group. I don't know. We still had joint seminars, both groups once a week. Though it was quite fun

in a way because we would get tape recordings from Calvin in Europe which were often played at the beginning (*of the seminar*) of what he was up to in Europe. For someone I hadn't met, it was kind of amusing. And then there would be some seminars. I never felt really out of depth in them so they weren't so focused in an area I didn't understand that it wasn't worth going to. What I did remember is that it started at eight o'clock in the morning.

VM: All the British contingent...

MF: ...noticed that. It wasn't quite so bad in one sense for me in that for the first two or three months my wife had a job in a firm that also started very early in the morning. I'm trying to think whether she had one or two jobs; one, at that stage, which started at 8:15 or something so I had to get up anyhow.

VM: She had come on an immigration visa so she could change her job?

MF: Yes. She subsequently changed her job so she could start at 9:00 o'clock in the morning at the YWCA, which we thought was rather a hoot.

VM: In those first four months you were really working in a room by yourself and really on your own little project...

MF: Yes, absolutely.

VM: ...with only tentative connections with anybody else?

MF: The connections in a scientific sense were more "where do I get this from?", "how do I get that", one or two rather extraordinary things in that...I mean, on the technical point of view, I knew how I would have done a separation in Oxford which would have used liquid oxygen as a coolant. That was thought to be incredibly dangerous there where liquid nitrogen was available. In order to get liquid oxygen I had to go over to Giauque's lab. where, in fact, there was someone working who I had known in Oxford, a girl, the first girl who ever worked with Giauque (he didn't believe women should be in science). They would give me these 5 litre globes of liquid oxygen which they were distilling and I could tap it out of the bottom of the still.

VM: Remind me: how much temperature difference is there between oxygen and nitrogen?

MF: Oh, I don't know: perhaps 15 degrees or something, critical for the separation I was doing. The thing that I wanted would not freeze in oxygen, and so I could pump it through, and it would freeze in nitrogen. The nice point was that they told me to be very careful, that when this had evaporated down to like 50 ml I was just to pull it out. The reason was that there was enough acetylene in the air in Berkeley that it concentrates in this stream and then you get an explosive mixture if you go too far down. So that was it. Now, if I had used liquid nitrogen under the system I was using, I would actually condense oxygen into it.

I worked by myself...very good for me in all sorts of ways...not what I had gone for. But in practice, I had already, from the last year with Hinshelwood, been an atypical student. With Hinshelwood I had an ICI fellowship even though I was working for a doctorate and that was pretty unique at the time. That, again, was one of the things, that if you worked with Hinshelwood you might get away with. I think that was the first time anyone had got away with it. But it was very nice for me financially.

VM: Sitting in Donner, you had some contact with ORL; you went in...?

MF: Only really for these weekly seminars. Gradually, as you got to know one or two people you would pop over and I got to know them that way. At that time, for example, Marilyn, his secretary, was actually in Donner, as was Norma.

VM: Were you conscious of any great difference between the Donner set-up and the ORL set-up?

MF: I think the Donner one was, in a sense, more physically instrumentally based and the other one was more the organic-bio; one certainly got that feeling. I was in the right place in that sense.

VM: OK. Then after four months, you...

MF: Then Calvin came back.

VM: Can you remember your first contact?

MF: I'm not sure I can remember my first contact. I can remember...it's the trivia you remember...the trivia I remember is going to his house, the whole group, to see his movies — which were awful. They had all the errors...I was a photographer and I had a 16 mm Rolex and Leicas and the lot. Calvin had all the errors of people who pan. My wife said it made her quite seasick watching these things. So that's the first thing I can really remember. Then I guess we must have had a discussion. He said, "Look: we've got this NMR machine" (it was actually a big Varian and, I think, the third ever made, and the second ever made never worked — that went off to South America and so clearly we were using the second ever machine). And Calvin said "we've got this machine; so and so is working on it, he's a physicist, why don't you go over and see what you can do with it?" That was my introduction. I went over and subsequently worked on NMR.

VM: The physicist was presumably Power Sogo?

MF: The physicist was Power Sogo. We were actually learning how the machine worked and all sorts of lovely things. I made a major discovery which subsequently put me off NMR forever because it was too late; someone else had made it. I didn't know that at the time.

VM: What were you trying to do with NMR?

MF: I think we were just trying to see what it would do.

VM: You were playing.

MF: We were playing. As it turns out, very unfortunately, I discovered the thing which was known as "hindered internal rotation". It was frightfully exciting. I spent quite a lot of time on it. I think we understood. We did it in a way which was really, in retrospect, extremely clever with the instrumentation we had. No one would think of doing it in this awful way now. I thought I was going to be famous.

VM: Was it published?

- MF: My stuff? No, no because I then found it in the *Journal of Chemical Physics* by Yankwich in Urbana who became famous for it and he was about six months ahead of me. But I had no idea that other people were working in the field or anything.
- VM: During this period in Calvin's lab., what was your attitude continuing to be like with respect to the family business? Were you thinking this was your last year as a chemist, or were you thinking there were going to be many more years?
- MF: No. It's extraordinary. I think one just puts off these sorts of things so you don't think about them at all. I can't remember thinking...all I can remember thinking of was what a lovely place this was and how nice the people were. I think I was thinking in all honesty that if I stayed in chemistry I was going to have to do something very different from what I was doing. I didn't see this as part of my career.
- VM: Were you concerned about acquiring publications while you were there?
- MF: Not at all.
- VM: Because you didn't see them as necessary to take the next step?
- MF: I think I was naive. I don't think I'd thought about things like that.
- VM: My memory of Calvin was that — I accept that you didn't see him for four months — was that he was very anxious to publish and publish quickly, because he was aware of the competition.
- MF: I think I was sufficiently orthogonal to what they were all doing that it didn't matter very much.
- VM: When you started doing NMR, was that not in ORL?
- MF: Yes.
- VM: Did that bring you closer to that group?
- MF: No, because Power and I worked together on this machine in a room all by ourselves.
- VM: So you still didn't terribly much mix with...
- MF: Nobody else was interested in NMR at the time. This was thought to be a sort of typical Calvin...If they had realised how incredibly important it was going to be as a technique, they would have thought differently. But nobody did at that time and it needed a hell of a lot more development. We just didn't have a field that was very stable. Everything had to be scanned in seconds and all the rest. I did publish something on NMR in the end the *Journal of Chemical Physics* or something, something like that, but it was trivial compared with what I had really discovered, but too late.
- VM: It wasn't a great publishing period.
- MF: I think I was young and stupid in one sense. Because having been so disillusioned that I had missed the boat, we didn't publish a whole lot of stuff which was publishable in that area and was very, very good indeed and wasn't actually, particular things there, found for another four or five years. It's just that when you think you have actually

made the major discovery, the things you think are less important are of no importance all of a sudden. That was very silly. I would never make that mistake 20 years later.

VM: As a young married, going to this rather luxurious accommodation in a new and exciting country for you, how did you organise your social life?

MF: Some of it was organised for us because it was an incredibly friendly group. As I say, my interactions were mainly social rather than scientific in all sorts of ways. There was a young chap, who had just got married there, Irv Whittemore, who, because we were of an age, we made friends with and have been ever since (in fact, their daughter is named after my wife)...

VM: Do you know where he is?

MF: Exactly where he is: he is in El Cerrito and we saw him last year when he came over.

VM: I hadn't realised.

MF: Yes, it's a great pity, that. But he...I'm trying to think what happened in his...but he left the group fairly early on and went to work for an oil company in Eel Cerrito and was an analyst for them and did very well financially anyhow. So that was one. Norma Werdelin also was a secretary and thought we needed looking after to some extent and did. The Lemmons: we would go skiing with them; that sort of thing. They were a mad skiing lot, as I'm sure you know at that time and we got introduced to it in all its lunacies — leaving at four o'clock in the morning and all the rest.

VM: I have to tell you that those very same people have been lunatic skiers until the last couple of years until they started breaking things a lot and had to stop.

MF: I understand absolutely; I see it in other people here. We might have gone on forever except that one came back to England and it's not that easy to ski here and we started a family and, you know, that was the end...we never went back. So our social life was built around the lab. It was so exciting that you could do quite a lot that wasn't in a sense social life; I mean food...the existence of frozen food and pies and things, I remember, just the whole business of shopping was so exciting.

We were very well looked after. When we needed a car, which we did immediately, Bert Tolbert immediately took over and said there is this chap up on The Hill in the Radiation Lab. and he has a second-hand Mercury which has been looked after incredibly well — it had its entire life history — which served us very well for two years.

VM: Did you travel?

MF: We travelled a lot in California, every weekend basically, I think, we went away. If it wasn't skiing it was doing all the other things.

I don't think it was deliberately, as it were, putting things off, but I think I must have felt at some stage that the work that I was doing there, such as it was, was certainly not central to any scientific career I had, if I was going to have one. That's why I then decided to spend another nine months in the States in Harvard. I'm pretty sure that I went to work in Harvard with Kistiakowsky on the recommendation of Hinshelwood. Kistiakowsky probably took me on that and the Fellowship could be extended for

nine months if you wanted. I spent nine months in California, three months travelling across the States, because that was part of the Fellowship, and then I spent nine months in Harvard.

VM: You didn't know Ken Sauer, did you?

MF: Yes, of course.

VM: He was with Kistiakowsky.

MF: Yes, we overlapped.

VM: He was there when you arrived?

MF: Oh yes. He was working for his PhD and I was a postdoc. I knew Ken well and he visited us here in England years and years ago.

VM: What did Calvin say? Did he say anything when you told him you were going to leave him and go and work with Kistiakowsky?

MF: No.

VM: Good luck?

MF: No. Oh yes. He was always...I mean, in retrospect it's kind of funny because my wife, and she only mentioned it the other day because of something else that had happened...my parents flew out to see us while we were there, to see that I wasn't beating my wife and things like that — they were very concerned. She had had this sheltered background, you know, and how were we getting on?

SM: Had you come out by boat or by air?

MF: Boat and train — lovely. My parents flew out and they flew out — God knows what it must have cost, because they flew out via Greenland in a plane that had beds. They went to sleep in beds, can you imagine? Anyhow, they arrived in California and I told Calvin that my parents were visiting. Of course, he invited us all for lunch and all the rest. It was very nice. I didn't think anything of it, which is sort of the free and easy way one has. But, you know, I was just a postdoc. starting and that's very nice, isn't it, a very distinguished chap, your parents are coming, "oh, you must all come to lunch". I'm trying to think whether I would have done it, you know, twenty years later!

VM: I think one of the factors is how young everybody was, including Calvin.

MF: That's right. Calvin was in his forties, that's true.

VM: He was not a stuffy man — not stuffy and only in his forties.

MF: I can't honestly remember...if he'd had been upset that I was leaving him to go somewhere, I'm sure I would have remembered. So I don't there could ever have been anything like that.

VM: Was the nine months that you spent with Kistiakowsky very different in scientific terms?

MF: The nine months with Kistiakowsky determined, in a sense, the course of my scientific career. It was a carry-on with things I'd done with...no...it wasn't a carry on with anything that I had done with Hinshelwood but it could have been. I could have done things like that with Hinshelwood but I hadn't for various reasons. I had done another pathway. What I did with Kistiakowsky, then I built on.

VM: The time with Calvin, therefore, was completely an interlude...

MF: Yes.

VM: ...and no follow-on scientifically?

MF: Other than I knew something about NMR and nobody else did, that was quite a plus and interesting in a way. And I think just going to America, that was the first exposure, I think seeing group working was an eye-opener for me and very important. In a way, I think I didn't like the thought of being a cog in a team.

VM: Before you'd gone or after you'd seen it?

MF: After I'd seen it. Before I didn't know anything about it at all. Of course, it's all the rage now to be part of the multidisciplinary team. I saw a multidisciplinary team and I think I saw more of the snags than the gains for me at that stage. Probably I have changed now and see the necessity.

VM: Interesting because I came a year later and was in the other place and for me Donner was the distance place. I was part of that team and there was no sense of being a cog on a wheel. The sense was entirely that I owned that activity, as everybody else did, and it was my personal interest that it should go forward because it was mine, too.

MF: I wasn't part of the team and it seemed to me extraordinary in a way. It seemed to me that Calvin was the chap who integrated everything; he was the only one who had any conception of everything that was going on whereas the ORL people really didn't know a lot of what was going on in Donner and to some extent vice-versa. Calvin could stretch from the physicists to the pure bio people in a fantastic way...

VM: Knowledgeably in your experience?

MF: ...knowledgeably, and I thought that was very plus.

VM: When he came back then, in the latter period when you were in his lab., did he visit you a lot in your...?

MF: He'd pop in rarely. He was very busy still with atomic energy stuff. We would see him, of course, at the weekly seminars.

VM: Did he want to go through in any detail what you'd done?

MF: No.

VM: Never?

MF: Only when we came to write up the paper.

VM: Was his name on it?

MF: No.

VM: So it was really an activity outside his main stream, as he saw it?

MF: I think at that stage, yes. Calvin had gotten an NMR machine with...I don't think with any real feeling of what he was going to use it for. He got it because it was a new technique and let's play with it and see what it will do. Maybe it will be something that will be important to use.

VM: Did he understand what it was or what it might be used for?

MF: I don't know. I am sure he understood what it was without any shadow of doubt. No, I don't think it was very clear. It's hard to remember...I mean, it's not hard for me but it's hard for people nowadays to remember how little was known about NMR. At that time I went over to Shell (*Development*) in Emeryville to hear a talk by a guy and they had been playing around with NMR for some time. They came to the conclusion that it was unlikely ever to be of any importance in the oil industry! That's absolutely wrong from beginning to end — it's one of their most powerful tools. But at that stage, you see, and I suspect with the sorts of resolutions and instabilities we had, they couldn't see all that was going to happen. All great people, and Calvin certainly was one, have these hunches, I suspect. I suspect he had a hunch that it could be important. Nobody at that time could have a hunch at how important it is now because none of the bits of NMR that we now have, two-dimensional pulse systems and all the rest that allow you to look at very big molecules, were even thought of.

VM: It was an extraordinary situation that he had that hunch. The hunch was enough to mobilise the resources to get the machine.

MF: Exactly, and they were extremely expensive machines.

VM: So he was a very powerful person to have acquired such a device on a hunch.

MF: Yes. I mean I think he was already someone who could get money without the sort of difficulties that most of us experience .

VM: Before we close, I would like to explore briefly what happened to the rest of your career. But are there more things that you would like to tell us that I haven't touched on...

SM: ...anecdotes, happenings, and things.

MF: They will come to me, of course, on the train going back home, won't they? If they do, I'll make a note.

VM: Or influences that the experience might have had on you.

MF: In the sense that I did stay in science, it clearly couldn't have been negative in any way. If it had been really negative, I'd have come home and gone into the family business, which my younger brother then did rather than me.

VM: Poor chap!

MF: He's retired relatively wealthy! There are anecdotes that were really to do with the skiing. The first time we went, you know, they all said "don't worry, we'll look after you" and this, that and the other. We got out of the car in wherever it was (Tahoe or wherever) and there were a lot of people, several carloads, and within seconds they had all disappeared except one who was delegated to look after us, get us into our skis, one discovers all these terrible things like there's no flat ground anywhere so as soon as you are standing on the skis you start moving, these sort of things. The thing that struck us as unbelievable was, first of all, originally that they got up at four o'clock in the morning or maybe earlier, to go and the second, within seconds of arrival, they were off! There was no sort of hanging around for a hot drink or anything like that. The enthusiasm was incredible. That really did strike us as quite strange. The other thing with the group was how sociable they were in that at the drop of a hat, they'd have a party, where we'd all go out for lunch together and this, that and the other. I thought that was very different to Oxford.

VM: And to Harvard or not to Harvard?

MF: I think Harvard was...yeah...I was with...I think the difference is kind of hard to say because, in Harvard, I was an "older" member of the group whereas in Berkeley I was a very, very young one. In Harvard most of the people in Kistiakowsky's group were research students working for their doctorates and I was a postdoc. and there weren't too many of us. So it was different.

VM: OK; let's expand a bit. Do you think the social atmosphere was unique to the (*Calvin*) lab. or was it a Californian phenomenon inasmuch as you could see, or was it an American phenomenon? In your later experience, can you narrow it to any extent ?

MF: No. I think it was a mixture. I couldn't see it happening...well, I think it required Calvin for it to happen. He was an integrating factor. But, of course, California was California and that was helpful, and America is America and that was helpful. In the absence of Calvin, there would still have been something; I doubt whether such a diverse group would have acted quite so much as a group without someone like Calvin. As I said before, he was the guy who actually knew everything that was going on. He didn't have...In a sense, of course, there was a hierarchy, but the hierarchy split very early on so it wasn't linear. Therefore, you did need someone like him. If it's linear, he doesn't have to know necessarily, someone else does. But he was the only one. I'm sure that whoever led the ORL people and it would be Lemmon...

VM: No, the ORL people was Benson and then Bassham.

MF: Sorry, yes; OK. I don't think they knew a great deal of what Bert was up to. I'm not sure that Bert knew a great deal (*about ORL*). But Calvin knew what everybody was doing.

VM: Several times you have said, and clearly we all recognise this, that Calvin was a great integrator. Was he also an originator?

MF: Oh, yes, I think so. In the sense, as we had said about NMR, why on earth he should have gotten...it can't just be because there's a new technique.

VM: No. There's a difference between appreciating a possibility maybe he heard from somebody else versus the sort of concept that he presumably had at the beginning of the photosynthesis thing, where he saw an opportunity and he saw how you could

take this opportunity forward with C¹⁴. That clearly is an original concept, the leap forward.

MF: I think that's right. I am too far away to see most of the things that he was very good at. But when he was young, he had some very nice work on triplet states which is totally different and, for me, a very exciting thing when I discovered it some time later, his name on a paper that I had looked up for a very different reason. I would never have known that. Of course, it became very important in all photosynthesis and in all photochemistry. Yes, I think he deserved his reputation.

VM: We're almost at the end of the...well, we can go on a little bit longer; there's still a few minutes. Having spent your nine months with Kistiakowsky, as you say, you then had determined that chemistry was going to be it.

MF: I don't know. When I was with Kistiakowsky I very much enjoyed what I was doing. Again, I was very largely master of my own thing. That's much easier in a university where the groups tended to be much smaller and nobody else was doing work on methylene which is the work I was doing. Ken Sauer was doing work that involved gas chromatography and I took that technique over from him and developed it in a rather different way and applied it to methylene. Now, the previous work on methylene had been done by a chap called Bruce Mahan who actually went as professor to Berkeley and died quite young, but a very distinguished chemist. Again, unfortunately, I had the horrid job of showing that some of his stuff was misinterpreted. But it was great stuff that I did and the two papers I published — I don't think Kistiakowsky's name was on either of them; it has thanks to him — were certainly among my best papers.

Tape turned over

VM: Yes; you said Kistiakowsky's names were not on the papers.

MF: I don't think they were. I would have to look back: it might have been on one of them and not on the other. And then I gave a seminar on the work in my last few days in Harvard and I remember Bob Woodward came and said to me afterwards that it was the best stuff he'd heard from a one-year postdoc. in a very long time. In fact, it was quite extraordinary in a way because I had applied...

You see, by then I had applied for posts in England. I was still talking in terms of "let's try it for a couple of years and see what it's like."

VM: Where?

MF: Well, I was saying that to my wife about coming back to England. "Let's try a post in university". At that time they weren't that hard to get, I suspect. I applied to four universities. One turned me down flat, that was Reading so I was quite pleased to go back there as a professor in the end; but, anyhow, that was the only one that turned me down. The other three places were Southampton, Liverpool and Newcastle.

Southampton offered me a lectureship sight unseen by telegram. That was really terribly awkward because I wanted to see what the other places were like, etc. So I said to Kistiakowsky, "couldn't we just pretend that this telegram hasn't come?" Kistiakowsky said "yeah, no problem, I will just put it in my drawer and send it to you in England". One mustn't do things like that because I got into a slightly tricky situation. As I say, Bob Woodward had come to my thing (*seminar*) and wrote off to

Cookson, who was the new professor at Southampton (who was a previous postdoc. of his), saying “take this man, Frey; he’s good etc. I’ve just come from a seminar.” I was due to say that the telegram, which had come two days before the seminar and Kistiakowsky was to write and say that I had left for New York where we did spend a week. So there was a minor frisson there about timing. But anyhow, it didn’t turn out to be bad in the end.

When I came back I then was due to be interviewed in Newcastle where they had kept the job open for about six weeks — they had interviewed everyone else — and then the Liverpool job, by the time... what Liverpool wrote and said was “terribly sorry, but by the time your application came, we had already offered the job”. By the time I got to London they phoned me to say that Barry Tracknell (*spelling?*) said he is leaving and you can have the job. AND Then I went up to Newcastle, and I thought of Newcastle as being in the Arctic Circle or something like that; it turned out to be really a very nice town, I was quite surprised. They subsequently offered me a job. so I really did, in the end, have a choice of three places. I chose Southampton because it was the closest to London, to be perfectly honest.

VM: Are you a Londoner?

MF: Yes. Basically, we said we have come back to England because our families are here. It would be very strange if we then go and live a long way away. So I took the Southampton job, which was the worst paid of the three, I remember. The work I then went on was a continuation of what I had started with Kistiakowsky and it was fairly important.

VM: How long were you there?

MF: In Southampton? I was eight or nine years: ’55...’57 I got there: nine years.

VM: And did you go to Reading from there?

MF: Yes.

VM: Where you’ve been ever since?

MF: Mm. Again you see...and gradually it became clear that I was going to stay in academia and not go into the family business; it became clear to my father, anyhow. He noticed.

VM: ...who offered the succession to your brother at some stage.

MF: Yes. I can remember after I had been at Southampton six or seven years, at that time there was none of this — “you’re good, we’ll make you a personal professor”. So I remember putting a compass on London and drawing a circle around it and then looking at *Who’s Who* at the ages of professors of physical chemistry and discovering there were only two places I could go to outside London and I knew I couldn’t go to London because we could never afford to live in London. Just because having got used to living on the salary we had outside London, the prices here were just so much higher that we thought we couldn’t possibly survive. The two places were actually Sussex and Reading.

VM: You said...you told us a little while ago that when you left Kistiakowsky's lab. you were coming back to England for a couple of years to see what it was like and you never went back to America. Why not?

MF: Oh, I think...first of all, we came back for family reasons. Then we started our own family. I think it was the thought...I think it was probably combinations, partly the education system — we wanted them educated here — and things grow on that and I think you make excuses. So probably we really didn't want to go back even though we thought it was fantastic. I think we must have made excuses because I did have offers.

VM: Have you been back to America?

MF: Many times.

VM: Now looking at it from the viewpoint of later in your career, forty years later, forty years after that, did you make the right decision? What does America look like to you now?

MF: Yes, that's interesting because, you see, even though I wanted to go to America, I was very anti-American before I went. I'm not sure that all young people at that stage weren't.

SM: You read the *New Statesman* faithfully?

MF: Well, I certainly was fairly left wing then and I gradually got more and more right wing, which is kind of funny, because when Kistiakowsky came over some years ago and I introduced him to someone, they said he was the only man they had ever met of distinction who had moved steadily left in his lifetime. I think the nice thing was we went pretty anti-American and we came back incredibly pro-American and spent a lot of time being very irritated with people (a) who didn't want to know about America. That's another thing about this country. You come back full of enthusiasm and you are welcomed back and great, and now can you please forget the last two years. We all go through that. I think that was very important for me that I actually became that because a lot of the science was there and one recognised that very early on. The number of times I went back to conferences in America was more than the number of conferences I went to in England and Europe, I suspect.

VM: Did you never go back for a sabbatical or an extended visit?

MF: No. I think if I had to run my career again that's the big change I would make. I think I suffered in all sorts of ways through not doing that, not developing the new techniques, which I should have done and all the rest. That was an error.

VM: Very briefly, what sort of chemistry have you been doing these last forty years?

MF: I went on, and on and off I have done methylene chemistry which I started with Kistiakowsky even to about five years ago. But then I went...Interestingly enough, Hinshelwood was famous for unimolecular reactions and I did nothing like that with him at all. But I then switched to unimolecular reactions after I came back, because of all sorts of reasons. That's really what I have done for an enormous amount of time. And then I did photochemistry. Quite interesting in a way, that I did do photochemistry because it was the early work that Calvin did and a lot of his photosynthesis derived from that, yet I never did any of that with Calvin. So, you

know, whether there was some subtle effect, I don't know because there was nothing anywhere else for me to do some photochemistry.

VM: At the time you were with Hinshelwood had he by then acquired his biological interests?

MF: Yes.

VM: But you were not part of them?

MF: No. Hinshelwood had two groups and they never discussed anything. We discussed things very rarely, I should say, as a group in the gas kinetics group. I can only remember twice ever in the whole two years having a group seminar with Hinshelwood. Only twice, in his room; he gave us sherry at the end.

VM: Was Alistair Dean there at the time?

MF: Yes.

VM: You knew him personally?

MF: Yes. I knew him very well but we never talked about his bugs work at all. But, of course, that was before I went to America as well. No, we never discussed the bugs work. It was a smaller part of the group; there was Alistair and two or three people who did bugs.

VM: It's interesting to think that if you had discussed the bug stuff and you'd been more familiar with it, you as a physical chemist, the different attitude you might have had to some of the work going on in Berkeley.

MF: Absolutely, it's quite extraordinary to come from that group and not carried any of it over. One was very isolated in that sense. As I say, you couldn't imagine Calvin running a group, even the size of Hinshelwood's, which, of course, was much smaller, and not having seminars.

VM: That's one of the things I found, and who came from England, so extraordinary about Calvin's group — maybe every other group in America but that was the one we saw. The breadth of interest and the interest that individuals showed in other people's activities. Something I had never known before and other people also hadn't known it. In a sense you hadn't experienced it either in Oxford.

MF: The two seminars we had in (*Hinshelwood's group*) in two years, one a year (not excessive), didn't involve the bugs people at all. I think we only had them because someone had run into real difficulties and Hinshelwood thought it might help if we all discussed it. It was so rare that I can remember one of them and what the problems were and the discussion and how intolerant I was.

VM: There's only one other thing, really, I'd like to put to you and I'm not quite sure whether you'll have very much to say. Many people that we have talked to have commented about the influence of that building (ORL) on the way the Calvin group operated. Did you observe enough of it to be able to get a sense?

MF: Not really. I have to...the only thing I can remember, and this is, as it were not answering your question but maybe is relevant, is that when I went to the opening of

the new lab., where the whole thing was built on a shape, etc. for interactions, I think I said to someone there “well, it didn’t seem to be necessary in ORL to design a building in this way to get people to interact”. It may have had an influence on me indirectly; I don’t know. It’s hard to bring all these things together. But when I first went to Reading, there was a sort of student magazine which was in its second year and as the new professor I had to do an article. They had just moved into a brand new building. I think I was not scathing but they were all head over heels about this new building, and I can remember very clearly saying that people were infinitely more important than the building. The people we appointed and how we then did is what would be remembered and not this smashing new building. You could do good work in a rather poor building and rotten work in a good building; it wasn’t a reason to have poor buildings! But I’m not sure that this didn’t have something to do with ORL because it wasn’t a great building physically.

VM: No, it was a building, however, which was moulded to fit the Calvin group’s mentality.

MF: I guess it might well have been. I think in that sense we thought Donner was very different. It seemed to me the “temple of togetherness” that was then built afterwards was a spanking building and it was based on a philosophy, but it never worked, I think, quite as well.

VM: Well, You are right. It was an interesting dilemma. The old building had been pulled down. Calvin succeeded in getting money to have a new one which he built. After all, there was this glowing, glorious memory of the old building which grew with each succeeding month after its demise. Here we were struggling to design something; we didn’t wasn’t to design a box. And so there was a distillation, we thought, and that was the best we could do in an attempt to recreate something on a different scale, in a different style but we weren’t going to rebuild the old building.

MF: And you see, there are some things that come into mind. You talk about effects on people, how has this influenced you and how’s that. There are also other things that are almost trivial but affect people’s lives enormously. Ken Sauer went there. When Ken was at Harvard, one of his pals was...I mean, one of the other people doing a PhD was Fred Tabbutt who subsequently actually came and did a couple of sabbaticals with me and that sort of thing. He had a daughter (now she wasn’t born in England. One of their children was born here while they were with us but she was the eldest) and she went and did a PhD with Ken Sauer.

VM: In that Round Building?

MF: Yes. Now that’s affected lots of people’s lives in these extraordinary ways. That happens a lot. There’s an enormous amount of scientific families, as it were, that people sometimes forget about. But they are enormously influential. So, that I have all sorts of other connections like one of Kistiakowsky’s students, long before I was there, was Sid Benson who went to UCLA, no University of Southern California and then SRI, and of the students from Cambridge who worked with Sid and then came and postdoced with me and is now a professor at Reading. Now, that has only come through because I did this and this, that they have come. They haven’t ‘said “Oh, you know; Frey is a distinguished chap, go to him”. What they have said is “I remember Frey, he worked with so-and-so; I was with so-and-so; he’s going to be all right; why don’t you go to him?” There were a lot of Calvin people like that.

VM: That is interesting. I think...have we finished? I think we've probably more-or-less exhausted the subject, unless there's something you can think of.

MF: No, as I say, if I can think of any anecdotes, I'll let you know. The snag, of course, is that it's a hell of a long time ago now.

VM: Well, I'll turn it off now.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name HENRY MONTAGUE FREY

Date of birth 27 FEB 1929 Birthplace LONDON

Father's full name HERMAN FREY

Occupation IMMIGRATION EXPLORER, TEXAS Birthplace ANTWERP

Mother's full name SARAH SEINGOLD

Occupation CURTAIN MAKER Birthplace LONDON

Your spouse LEILA ANN FINN

Occupation SECRETARY Birthplace LONDON

Your children JEREMY GRAHAM FREY

COLIN LUNNEL FREY

Where did you grow up? LONDON

Present community READING

Education HARROW COUNTY; OXFORD UNIVERSITY

Occupation(s) PROFESSOR

Areas of expertise PHYSICAL CHEMISTRY

(GAS KINETICS)

Other interests or activities Photography, Opera, Theatre.

Organizations in which you are active ROYAL SOCIETY OF CHEMISTRY,
ROYAL BURNHAMPTON & BATTLE HOSPITAL

Chapter 40

CAROL (QUARCK) GRISEBACH

Freiburg im Breisgau

March 10th, 1997

VM = Vivian Moses; CG = Carol Grisebach

VM: This is talking to Carol Grisebach on the 10th of March, 1997 in Freiburg.

Can I start by asking how you came to be in Berkeley, in Calvin's lab.?

CG: Yes, you can. After I finished my education at the universities, I went to work for Eli Lilly.

VM: Which university was that?

CG: The University of Wisconsin; before I went to Duke but this was Wisconsin. I enjoyed that part of my development very much because a chemist named Kurt Gerzon, with whom I am still exchanging letters and he is still as active as ever, I learned a great deal from him. We had also private conversations which were very important for me, too. But the thing is that Indianapolis is, for me a dead place, there's practically nothing going on there for young people. After a while, I thought if I stay here I'll be buried here more or less. I just felt there were just a few young women that were in this laboratory, all the men were married. Eli Lilly always saw to it that they were married before they hired them because it's easier. So then I told Kurt my decision. He was very kind about it and said "well, I'll be sending you material on what the latest thing on erythromycin so that at least you can keep up with that story when you go. And actually I hadn't met...that was actually right after I had met Professor Calvin and there was some kind of meeting there and we were allowed to go there in Indianapolis.

VM: What had your background been?

CG: Chemistry.

VM: Straight chemistry?

CG: Straight chemistry. I attended this meeting and I remember Melvin Calvin turning around, we had been talking about it, and I said "oh that (*Berkeley*) must be a wonderful place to live in and work in." And he said "well, just send in your

application” — that was a sort of a little bit...just a side comment we were having and I said “well, I think I really may do that.”

VM: Did you take him seriously?

CG: Yes, I took him seriously. He said you just come. I said that we were planning a trip to California with three other Lilly women and everything went smoothly. I got the position and after that, that was the lab. that I worked in and that’s where I met my husband on the very first day. I had driven to California all the way from Eli Lilly and I walked into this laboratory, the old one, of course, and there was this young man, very tall — he just smiled at me as if he had always known me. It was all sort of almost like a Cinderella story. Everything worked out the way I had hoped it would. Then Hans (*Grisebach*) had to go back (*to Germany*) because he had only a year’s leave. He had a scholarship from NASA. That was up and he had to go back to Germany.

VM: The space agency?

CG: Yes. I doesn’t mean anything usually about your own work. Then I continued to work in California for...I was there for three quarters of a year altogether.

VM: So Hans had gone?

CG: He had to go. He had only the scholarship and he went back to Germany./

VM: How long after you got there?

CG: Very soon... about a month, I think.

VM: So, your attachment was a bit whirlwind.

CG: Yes, yes, yes, yes. But he said “now, see that you can come over”. So I applied for a scholarship to a German-descendent professor who was still there and enjoyed talking with me because I knew people in Germany. Now I’ve forgotten...what is his name? I went to him because I needed references and he immediately gave me one and talked with me some time about Germany.

VM: Can I backtrack a bit? When you were at Lilly you were already postdoctoral, were you? It was your PhD you did in Wisconsin?

CG: No in Wisconsin I didn’t; where did I do my PhD? That was later, I thought. I’m getting confused. Before I went to Lilly I was at the University of Wisconsin and there I told my professor that I didn’t want to get a PhD there; it was just too much for me. Later I got a PhD but I’ve forgotten how that went, how I managed that; I think in Germany.

VM: Did you not mention Duke?

CG: Yes, I did undergraduate work at Duke and graduate work at the University of Wisconsin...

VM: But not for a PhD; that was a master’s degree?

CG: Yes, I stopped with a master's there. Then because Hans said that in Germany it would be very much better if you had another degree, that's when I then also: I went to the University of Munich. Not, not Munich, excuse me: Berlin. At that time there were still circumstances and you couldn't tell where they were going and it was just luck if you got a position and your husband got a position. His (*Hans's*) professor had a lot of pull and he had specifically gone to Berlin so he got me a position there too. And that's when I got my degree.

VM: You don't mind me asking questions?

CG: No.

VM: I will ask for Hans as well because unfortunately he can't answer for himself.

CG: Of course.

CG: He was not yet finished and when he came back to Germany he went back to his old professor and got another degree (I can't remember the names of the various steps in the Germany system). At any rate he got to be a full professor. I'm trying to remember where he worked after he finished up in Berlin. We were married fairly soon after I came to Germany and for some reason we landed in Freiburg but I can't remember what it was, why we landed there.

VM: What was his status when he went to Berkeley? Why did Hans go to Berkeley?

CG: I think he was not yet *habilitiert* — what they call *habilitiert* but he was working on it.

VM: So he didn't actually have a permanent job to come back to.

CG: No, no, not to the US; not at all.

VM: And when he came back from the US?

CG: When he came back from the US he went back to his old professor and he...I don't know whether it was just *habilitiert*, that was one step; I can't remember what all those were called. Anyway, he got to be a full professor; he had what you needed to become a full professor.

VM: So he'd been in Freiburg before he went to...

CG: No, he hadn't been to Freiburg at all...after we got to know each other...

VM: It's all so long ago.

CG: It's all so long ago. He went...I'm trying to remember where he worked then. When we came back to Germany, we were married first and for some reason we landed in Freiburg but I can't remember what it was, why we landed in Freiburg.

VM: And why did Hans go to Berkeley?

CG: I think he told it was a very nice place to be. I don't know about Calvin; I don't know how much he knew about Calvin.

VM: Was he a chemist?

CG: A chemist.

VM: A straight chemist; not a biochemist?

CG: Straight chemist. He did this other work; I don't know what was considered at that point. He was doing this work when he went to Calvin. There he changed and did that work so you could call him...

VM: His background was that of a chemist.

CG: Yes, an organic chemist.

VM: I now have both of you in Berkeley. Obviously you don't remember what it was like when Hans first went there but you remember what it was like when you first went there because you saw him when you walked through the door. What happened as far as your work was concerned? How did it happen that you started working on some particular project? Did you talk to Melvin; who else might you have talked to?

CG: Al Bassham. Al Bassham was sort of in charge of our laboratory. He was having a problem with his wife and he did divorce her then married this young girl who was in the laboratory (*Donner Lab.*); he was much happier after that. Well, I remember coming into the lab. and seeing all of these people, Hans especially. We did an awful lot of chromatograms to get that missing link. Alan Barker was also involved with that...

VM: Yes, we hope to see him within a few weeks.

CG: Say "hallo" and please give him my regards. And he was sort of my superior...well, we didn't have superiors...

VM: Your mentor.

SM: Mentor: I had to be introduced to all these things. So he showed me what to do and I remember looking for...but I don't think we ever found it.

VM: What was the missing link you were looking for?

CG: It was some kind of product, we thought — or he thought — which was being produced in some kind of chain and we were trying to find it on the...is it a chromatogram?

VM: You mean the X-ray film?

CG: Yes.

VM: The radioautograms.

CG: Yes, I think that was it. We couldn't find it where it was supposed to be once you ran it. I don't remember how that turned out because that was pretty close to the time that I was already leaving.

VM: But you did publish some papers there, at least one (I don't know how many) because I saw your name, when your name was Quarck.

CG: Yes, Quarck, right.

VM: And you published under that name. And I don't remember how many. It's in the record books.

CG: It's not a great deal because I went to too many places, actually, you know, to get my name...But on the other hand, I learned a lot that way and each stop that I had (Wisconsin [university] and practical things going on at Eli Lilly): that was especially now because of Kurt Gerzon, a very kind of special person.

VM: How do you spell his name?

CG: G-e-r-z-o-n. He was a Dutch Jew that emigrated just in time. I guess he got over to...he lived on one of those islands, a Dutch island because had to leave Europe; he never talked about it. He always tried to have something positive. We had all sorts of discussion but he always managed to see to it that everything was done, that we didn't get off the track. It was pleasant for me because I learned a lot. I just thought he was a very, very nice person, and very, very cultivated; I mean he knows a lot. I still correspond with him; about a month ago he wrote me a long letter, several pages, and what's he into? Now, of course, he's retired but he's still as busy as ever.

VM: Can I take you back to Berkeley?

CG: It's because that bit came first, you see, before Calvin.

VM: You worked the whole of your time in Berkeley in ORL, the wooden building?

CG: Right; yes.

VM: Working in the group with Al

CG: Yes.

VM: Did you work with Hans directly?

CG: I don't think so. He was doing something else — but what was it? Oh, I know: that acid...

VM: Lipoic.

CG: Lipoic acid. Yes, that is what he was working on.

VM: Can you remember...you already remembered Alan Barker and Hans being there. Can you remember who else was in there while you were there?

CG: The girl, Marilyn...

VM: Marilyn Taylor.

CG: She was always coming. There was a couple, also I think from England — anyway, they had an English accent, a very tall — Monty, his name was.

VM: Oh, Monty Frey.

CG: Yes — and his wife.

VM: Don't know his wife, yes. Saw him quite recently.

CG: They were very nice and interesting because Calvin had such a nice way of integrating, I mean of calling a meeting and having the girls...what do you call them? They're not assistants in the laboratory...?

VM: Technicians.

CG: ...technicians and they...Professor Calvin always insisted that they take part even though they didn't people into the lab., he would call a meeting and having the technicians take part as well even though they didn't understand most of it . I was very impressed by that. I am sure that it worked favourably on their feeling for the lab., I think it probably really paid off in that way.

VM: Was his lab. very different from what you had previously experienced at Lilly and Wisconsin?

CG: Well, let's say, yes there was a difference. Because at Eli Lilly they had found certain things and this was research, their department was research and a lot of interesting things came from that. Dr. Calvin, now, always had these group meetings, everybody was sort of involved and he was always so positive, he was sure that things would be coming up the way they were hoping to do it and so forth. That was a very nice atmosphere; whether it was always that way, I don't know, but it just made the atmosphere very pleasant. He was that way all the time. When I first came, when I first met him, he was that way at that meeting— I said “that sounds wonderful and he said “yes, yes, come out”. You know: without even...that was his way.

VM: Did he come into the lab. often? Did he talk to you often when he was there?

CG: No, I don't think so. But, of course, I wasn't there very often; it was usually Al Bassham. You know, he was my boss, more or less; he had been there a long time.

VM: Had Andy Benson left by the time you were there?

CG: Yes. He was gone and he must have been very popular because a number of the women were always mentioning him.

VM: He would be pleased to hear that!

CG: Where did he go? Further south?

VM: He is now in La Jolla. He has been in La Jolla for 33 years, he told us, but he had some intermediary stops in Pennsylvania and Los Angeles before he landed up there. He has done very well since then. I have been in touch with him quite a lot recently. What about the social atmosphere in the lab.?

CG: Let's say, nobody was standing around gossiping or anything like that, I would say. We had these weekends that really were open to everybody: somebody said “I'd like to go skiing, too” or “I'd like to do something” and everybody usually would be willing

take another person in a car and drive out to those places that have enough snow...forgotten their names...

VM: Up in Yosemite and places like that.

CG: Yosemite and also a couple of other places that we all went.

VM: Squaw Valley.

CG: Squaw Valley. The we would spend the night there because it was too far and I remember that young girl that married Al Bassham then...

VM: Her name is Leslie.

CG: Leslie, right, and that was where this developed. You could see that this was going to...he was already divorced. I haven't heard anything about them or seen them. I haven't returned (*to Berkeley*), as a matter of fact. I was in...I wasn't in Berkeley, somewhere else once since I'd been there. I forget with whom I was...after Hans...I think, to visit somebody, I don't know. But otherwise, I think that's about...there's not too much more that I can talk about.

VM: So were you one of these people there, as it was in my day, in my period, who worked day and night, as it were, weekends when they felt like it, or were you less dedicated?

CG: I pretty much followed what Alan Barker was doing. He was really into everything. He had to learn himself but he was always there in the morning and I felt I could ask him something. But, of course, he wasn't near as far along as the others were.

VM: He was there when you got there, was he?

CG: Or else it was almost the same time. I remember we had a big party at somebody's house, I think it might have been mine, I don't know. I had rented one with a girl friend there. Anyway, I remember that. Everyone was very jovial, we had a very good time. Of course, he had a very strong accent.

VM: Alan Barker? Yes; I think he still has.

CG: I don't think he could lose that; it was probably too late anyway. But I understood him. He had a very nice wife. That was always nice, I thought, that everybody was included so that there weren't wives sitting around for a year being sort of shut out of things. Again, I think that goes back to Calvin, how he reacted. There was this very tall Swedish-looking man; now what was his name? He was still there when I left.

VM: The name I can remember is Arnold Nordal...

CG: No.

VM: Goran Claeson.

CG: No. He must have left earlier then.

VM: In that case, I don't know.

- CG: No, he must have left earlier. I can't remember what his name was; it was very Scandinavian. He was very tall and I think his wife was very short or something like that; I can't remember.
- VM: You don't mean Ozzie Holm-Hansen?
- CG: Yes!
- VM: Oh yes. He became very friendly with me. His wife was called Harriet.
- CG: Right; Harriet, yes. Right, right, right.
- VM: They are now divorced and, in fact, have been for many, many years. He is in La Jolla as well.
- CG: He's in La Jolla? Uh huh.
- VM: He long outlasted you. He stayed there until about 1959, 1958 or 1959, I can't remember exactly.
- CG: Yes, yes.
- VM: He spent three years there. Yes: he's a very nice fellow.
- CG: Very nice; yes, yes. So gradually, I'm getting practically everyone that was there.
- VM: Now I can begin to get you more accurately. You must have arrived there in 1955, in that year '55/'56 and I am sure that Ozzie came a year before I did and I was there in '56. So that means you ought to have a bit overlapped with Rod Quayle.
- CG: Wait a minute...Rod Quayle...a little before. Is he from England?
- VM: Also a tall fellow. He had to be a year earlier than; that's quite true.
- CG: I don't know whether I saw him or not.
- VM: Do you remember a young man called Karl Lonberg who was a graduate student?
- CG: Yes; he was a graduate student.
- VM: You worked in one of those big rooms, did you, in ORL with lots of people coming in and out?
- CG: Yes. People coming in and out. It was all very harmonious. Everybody wanted to do his best, I think, there. The atmosphere there...
- VM: It wasn't...
- CG: ...so much competitive, that's very important.
- VM: I think one of the factors is that people were very young comparatively and that many of them were people who were passing through and weren't staying there very long. That was a step along their path of life and they weren't competing with anybody.

- CG: Oh, I think definitely, definitely. It was a wonderful atmosphere. Then there was the secretary, Alice...
- VM: Alice Holtham.
- CG: Alice Holtham.
- VM: She now lives in Seattle; we visited her last year.
- CG: Was she married?
- VM: Yes; she married someone called Ernie; he name is now Alice Lauber. She married a man Ernie — Ernie Lauber — and they live in a house in a suburb of Seattle.
- CG: I have a nephew there and sometimes I go there. And who...?
- VM: Was Alex Wilson one of your contemporaries?
- CG: No, I don't think so. I don't remember that name.
- VM: Well, I'm beginning to place you in the scheme of things. I think I have pretty well established when you must have been there. Hans got there essentially a year, or the best part of a year, before you did, you just overlapped at the tail end.
- CG: Overlapped for just about a month. But we had a lot of fun. I remember we went down to that nice place on the beach down there; we went there with another couple on weekends. There's a beach on the ocean...
- VM: ...near San Francisco?
- CG: Yes.
- VM: Stinson Beach?
- CG: It doesn't really matter. You can get to it fairly quickly and has a nice beach. Then there was another one — there was a golf course there and we all had our picture taken with a fantastic car, I remember that was a big joke. Hans had a really old car, because he had no money, of course, when he came there. He had this old thing; we were always afraid that it would break down before we got back.
- VM: Everybody ran these old jalopies, didn't they? None of the postdocs. or students had much money did they? They had enough and they all had old cars; it was fun...they broke down from time to time but it wasn't too serious.
- CG: I saw pictures of that.
- VM: You don't remember where you lived in Berkeley, do you; which street it was on?
- CG: No. I know about what section it was: very close to campus. I moved because the first one, the woman that was renting that out was not too pleasant. Then I roomed with another but she wasn't in Calvin's laboratory., she was doing something else in California. We shared that apartment; that was a lot better than the other. We did a lot of hiking. It was a lovely time, it was a wonderful time.

- VM: When you moved on, he (*Hans*) came back to Germany and you eventually joined him here. Did you start out in Berlin? Was that where you first got together?
- CG: I don't know whether I had been...no, I hadn't. I had been to Europe with my parents for a visit, but I hadn't been there. I think that Hans went back to his professor...
- VM: Who was were?
- CG: ...in Berlin.
- VM: I see; Hans had started in Berlin.
- CG: He had started in Berlin
- VM: Was he a Berliner himself?
- CG: I think he might have been born there, I think he was. Then, of course, very quickly there were terrible situations. He never said much about that. Anyway, by the time I came over, that was over. That was Professor Weygand.
- VM: Oh, he was in Weygand's lab.?
- CG: Yes, in Berlin.
- VM: So he went back there from California and so when you came over to join him and get married, you were also in Berlin?
- CG : Yes. I was in Berlin.
- VM: Did you work in the same lab. as him, then?
- CG: Yes.
- VM: I see. And eventually you left Berlin and came here —directly?
- CG: Yes.
- VM: Have you been here ever since?
- CG: Yes.
- VM: So you've been here for a pretty long time now. Did you work here as a scientist?
- CG: In Germany I worked for a while before my children were born; I was mainly in Berlin because I was already fairly well along (*pregnant*) when I came here (*to Freiburg*) with the child I was carrying. So I never started in in Freiburg at all. I just finished up what I could do in Berlin and than I wanted to take a break but it turned out that I didn't go back.
- VM: You never practised science again?
- CG: I didn't practise again. Hans was very successful and here he became a major professor which he liked; I mean, he liked the university atmosphere too.

VM: He was a professor of plant physiology?

CG: Professor of plant physiology. I think that his background in organic chemistry I'm sure helped him. He wasn't strictly...he had not, I think, thought he would end up something more in that direction, but that is what they wanted here in botany and the botany was behind times. They said "no. no. no; what we want now is this and that" and it wasn't difficult for him to change over. His background was so broad he could apply that to almost any direction in this case. So he was very happy and had first class people working with him, also as full professors...one was...all related subjects. They all got along very well. They were all about the same age. They got together; they agreed, they didn't squabble; you know, try to take things away from him. It was a very, very nice time; it hasn't always been that way all the way but with this group it was.

VM: When he came back from America, did he feel, as far as you remember, did he feel that the German atmosphere in the universities was very different from what he had experienced in America?

CG: He never mentioned that, really . He never really talked about that.

VM: Well, what did you feel? When you first came here as a resident, not just as a short-term visitor, and began to work in the German environment did you find it very different from the American scene?

CG: Yes, to quite a degree. For one thing, it was still sort of post-war situation and they had to make do with things that we would not have had to in the United States. But Weygand was able to get a lot of what he needed. I was used in Berkeley to talking with the people just between times or at breaks, something. In Berlin people didn't talk to each other that much. I remember Hans coming out of the door with somebody, somebody...they had been working on something...they had an argument with somebody else and they were talking about it but it wasn't...apparently it was good work being done; I was not in that field when they were doing. I think that Hans felt that this was the way things always were here. I was a little surprised. First of all, there was this one major professor that had so much and he got all these things that he could do the work with. Of course, he was in a better situation. Hans enjoyed being in Berlin and I can't say I didn't. They found a place for me in the laboratory.

VM: When I first went to America, to Calvin's lab., I found the atmosphere startlingly different from what I had been used to because England was post-war: it was dull and it was formalised much more than the Americans. There was this sudden, this free and easy free-for-all, everybody talked to everybody else and there was no hierarchy or rank. I wondered whether you, being an American and coming to into a European, particularly Germany which is probably more rigid than England, whether you'd really noticed a great change.

CG: I definitely did. But the fact that I was engaged to Hans — I might already have been married — that was so important. I could always go to him. He was always in good humour, if I had problems and then there was another professor who was sort of a little funny and we made fun of him because he was not taken too seriously, but he would come in with great ideas or something and I thought that was great. You know, we would talk about it and he was so overly optimistic, he was so optimistic that people just smiled about it. But he didn't care; he said "OK, I was right, you were right", or something. There were a couple like that. And then there were a couple of other girls, I remember, but nothing compared to Calvin, nothing.

- VM: Did Hans ever send any of his students to Calvin, as postdocs. or as graduate students or anything of that sort, that you know about?
- CG: At the time he was still doing what they call his *habilitation* so he was not in a position to send anybody. His professor, that he was still with, would have to have done that.
- VM: But later on, when he was here, when he was in Freiburg?
- CG: I don't think he ever had anybody that was interested.
- VM: The only person I can remember, during the period I was there and that was, after all, several years, was a man called Hans Ullrich. Do you know him?
- CG: Yes. Hans Ullrich.
- VM: I don't know whether he originated in your Hans' lab., I don't know where he came from. I think he's here now.
- CG: He's here. Ullrich, of course. Hans Ullrich. But I haven't heard anything about him. You think he is in Freiburg?
- VM: He was from Freiburg and he came to California in the early '60s, I would say, and spent a year and went back. I have to say I haven't had...I've no contact with him since and he is a bit "late" for my period because I have to stop some time. He's the only connection I know with Freiburg and I wondered whether Hans Grisebach had had any prior association with him; whether he had been one of Hans' students or postdocs. or what. But you don't know of any connection?
- CG: I don't remember. Now that you say the name, I remember Ullrich. It could be; I would have to ask somebody here in the Botanical Institute.
- VM: I don't want to — I mean, I'm perfectly happy to go on, if you remember any more. Are there incidents or stories or "funnies" or whatever that you can remember that we haven't talked about that might be interesting?
- CG: I should have thought about that more intensively before. I was always surprised how congenial everything was and I remember in the lab. there was very little conflict. Everybody would come for coffee at a certain time but then when they'd had their coffee they went back to their labs. and...You always enjoyed going to the laboratory. It was something you wanted to go to, not something you had to.
- VM: You remember that there was another section of Calvin's group in the Donner Lab.?
- CG: Yes, there was, but I had very little contact with anyone there.
- VM: That's what I was about to ask you whether you had.
- CG: No. Well, wait a minute: now there was this very tall man, what was his name, now that you say Donner Laboratory?
- VM: Dick Lemmon.

- CG: Dick Lemmon. Right. Dick Lemmon would quite often...he'd always be invited to the parties we had.
- VM: He's a keen skier as well. His wife's called Marguerite. And Bert Tolbert: do you remember him?
- CG: Of course, everybody knew about Bert Tolbert. Did he ever get married?
- VM: Oh yes. He married a woman called Anne Zweifler who was in the lab. as well. But I'm not sure when that was, whether it was after he left Berkeley or before. I don't know. It's probably in the record somewhere.
- CG: Yes, yes, yes, yes. He would go on ski trips with us and things like that I remember.
- VM: You certainly have a warm feeling about that part of your life.
- CG: Very much, very much.
- VM: I think really it remains only to thank you very much for putting up with me intruding on your life, not meeting you at the railway station (*because of construction work*).
- CG: Well, that wasn't your fault.
- VM: I'm afraid it's one of those things.
- CG: I was unprepared for that, too.
- VM: OK. We'll stop at it now.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Carol Grisebach

Date of birth May 8, 19 Birthplace New York City

Father's full name Rolf Quarck

Occupation Head of his own company Chemtax Birthplace Hamburg, Germany

Mother's full name Kaete Quarck

Occupation House wife Birthplace Hamburg, Germany

Your spouse Hans Grisebach, deceased

Occupation Professor of Chemistry Birthplace Silesia

Your children Franziska Langbein and Rolf Grisebach
Now Housewife in Leipzig, formerly pharmacist management consultant

Where did you grow up? Port Washington, New York (Long Island) USA

Present community Freiburg, Germany

Education Ph.D in Chemistry

Occupation(s) Worked as a chemist in the Lilly Laboratory
in Indianapolis, then in the Calvin Laboratory.

Areas of expertise Chemistry of Erythromycin many years
ago, but not any longer.

Other interests or activities Traveling to other countries

Organizations in which you are active For many years I have led
an English discussion group in which German academic
women practice speaking about interesting topics in English

Chapter 41

HELMUT SIMON (with Hildegard Simon)

Freising

March 15th, 1997

VM = Vivian Moses; HeS = Helmut Simon; HiS = Hildegard Simon

VM: So this is talking to Helmut and Hildegard Simon in Freising on the 15th of March, 1997.

Helmut, can we start by my asking you how you came to go to Calvin's lab. all those years ago?

HeS: Actually the reason was that in the beginning of the '50s the ideas of Calvin about the path of carbon and photosynthesis became known and, of course, the techniques were very interesting and so on the occasion that Calvin came to Berlin, I think in '55, I asked him whether there would be a chance to work in his lab. and he told me if I would find some money he would be happy to take me and so I applied for a Fulbright grant and it worked and so we arrived in '56 in Berkeley.

VM: What had been your own background up to that time?

HeS: I was trained as an organic chemist and in my thesis I worked on the synthesis of carbon-14-labelled compounds and also using these carbon-14-labelled compounds for solving some problems like biosynthesis of certain natural products, for instance such as pterines and so on.

VM: So you knew about Calvin's book on isotopic carbon?

HeS: I knew about this book, I think, from '47 or '49, I don't remember at the moment. I read it with great interest already when I started my thesis in '52.

VM: And where did you do your thesis — in Berlin?

HeS: No, I did my thesis in Heidelberg.

VM: So what were you doing in Berlin when you met Calvin?

HeS: After finishing my thesis in Heidelberg, I went for half a year to Tübingen together with Professor Weygand and since Weygand went to Berlin afterwards I joined him. So since Spring '55 I was at the Technical University in Berlin.

- VM:** When you had met Calvin in Berlin had there been any discussion of what you would do in his lab.?
- HeS:** No, there were no detailed discussions about the work what I should go to do. Only he agreed that I can come to him.
- VM:** Right. And you were already married at that time?
- HeS:** We were married already.
- VM:** How did you travel to America?
- HeS:** That was a very interesting 10-day boat trip from Bremerhaven to New York. Then we stayed a fortnight in New York and then we flew from New York to San Francisco.
- VM:** And that was the first time you had been in America?
- HeS:** That was the first time that we had been in America, yes.
- VM:** What impression did America make, particularly San Francisco?
- HeS:** Of course at this time with the background of the destroyed Germany with all the restrictions there, and this wealthy country...that was a tremendous difference and we were extremely impressed about America.
- VM:** Had you been outside Germany before then?
- HeS:** Yes, but only in Europe, Switzerland and so on.
- VM:** So you came to San Francisco, at the airport. What did you do — was somebody waiting for you?
- HeS:** Yes, somebody was waiting for us but that we learned the other (*next?*) day. Actually the plane was too late, I think by about two hours due to strong head winds, and the flight was maybe thirteen hours or so at this time and we had the impression nobody was there so we took a bus and rode to San Francisco. We were extremely hungry. I expected, as I was used in Europe, that during such a flight food would be served and we had no food from New York to San Francisco — a lot of coffee and things like that but no food. So the first thing we wanted to have something to eat and we went into a restaurant or something like that (it was in the meantime maybe half past ten in the evening) and we sat down at a table waiting that somebody will come and ask what we want and nobody came. Since it was late we were the only guests for awhile. Then came a group of other people and we saw what they did. They took a tablet and had a self-service so to say. Then we knew what we had to do.
- VM:** So you were not hungry for too long.
- HeS:** Ja, but it was a little bit frustrating.
- VM:** Did you go to Berkeley that night?
- HeS:** No, we looked for a hotel in San Francisco. We thought about calling Professor Calvin but then we thought it was already too late and so we just looked for a hotel,

stayed there and next morning we called him and he was really happy to hear us and told us he that sent Paul (*Hayes*) and then we heard that there he asked for Dr. Helmut by loud speaker but we didn't know it had something to do with us. And he told me we should wait for a bus in an hour or so and somebody would pick us up. Then we were brought directly to Calvin's house. It was at noon time on a Saturday.

VM: And you stayed with him for...

HeS: ...a couple of nights, three nights or so.

VM: And then somebody helped you find an apartment?

HeS: Ja. Calvin was very helpful and Mrs. Calvin. They drove us around and we looked at two or three apartments. It was actually a matter of two or three hours to find these apartments. It was extremely surprising for us — the situation in Germany was quite different as there it was a question of months to find an apartment.

VM: Exactly the same for us when we came around the same time. When actually did you arrive in Berkeley? Do you remember which time of the year it was?

HeS: It was maybe the end of July or August.

VM: Did you start working straight away?

HeS: Ja.

VM: How did you decide what to do — who did you talk to?.

HeS: Calvin made a suggestion. He had an idea that the biosynthesis of glutamic acid, that is glutamate, may be influenced by an antibiotic (that was azaserine) and the idea was to prepare glutamic acid in illuminated *Chlorella* in the absence and the presence of azaserine and degrade it and to see the difference was the carbon-14 labelling.

VM: How much experience had you had of this technology before you arrived there?

HeS: To this kind of technology I was used — degradation of products and determining carbon-14 distribution . That was due to my thesis.

VM: But did you prepare your own photosynthesis experiments in order to get glutamic acid which had been made in this way?

HeS: Ja. I did my own experiments, ja.

VM: So you had to learn the whole Calvin-type of photosynthesis techniques.

HeS: That was what I had to learn.

VM: Did you work closely with anybody while you were in the lab.?

HeS: Not in the first couple of months. I was concentrated on this glutamic acid-glutamine business. Later I came in cooperation with Metzners and partially also with Otto Kandler.

VM: Was Nel van der Meulen also not working on this azaserine problem?

HeS: As far as I know not at the time when I was there.

VM: Was she there at the same time as you?

HeS: I don't remember her.

VM: Maybe she took it later. Well we'll see her in a few weeks' time and we'll talk to her about it. And so, did you work on this problem the whole time you were in the lab.?

HeS: I think maybe after about six or seven months (altogether we were there eleven months) this problem faded away and I did more and more experiments together with Metzners.

VM: Yes. On methyl phosphate.

HeS: On such an unknown labile compound which is formed by illuminated *Chlorella* cells.

VM: To come back to this glutamic acid and glutamine, what happened with the work that you did? Did it show anything, anything interesting?

HeS: Not a very clear result. Actually, Professor Calvin wrote a paper and delivered it, I think, to BBA (*Biochimica et Biophysica Acta*) but they didn't accept it as it was and they suggested a couple of more experiments. But in the meantime I was already back in Germany and Calvin sent me their reply and asked me whether I could do these experiments but there was no chance for me to do these kind of experiments in Germany so nothing happened, actually.

VM: You worked, as I remember, in the Old Radiation Lab.

HeS: Yes, in this old...

VM: In the big lab., with other people?

HeS: In the lab. before this big room with the centre table, ja.

VM: Do you remember who else was in the room with you?

HeS: Actually I don't know exactly who was where but the people who were present in the Old Radiation Lab. were the two Englishmen (Vivian Moses and Bob Rabin), then the Swiss...what was his name?

VM: Utz Blass.

HeS: Utz Blass. Then van Sumere and there was a Chinese Ph.D. student but I forgot his name.

VM: You mean Ning?

HeS: Maybe Ning.

VM: Ning Pon.

- HeS:** Oh, ja, Ning Pon. He was a little bit older, I think. He was present but there was also an additional younger...
- VM:** Mel Look.
- HeS:** Maybe, ja.
- HiS:** And Rosemarie...
- VM:** Oh, Rosemarie Ostwald?
- HeS:** Rosemarie Ostwald? Wasn't she working in the Donner Lab.?
- VM:** I think she was in the Donner Lab.
- HeS:** She was in the Donner Lab. working, ja.
- VM:** And do you remember the secretaries there?
- HeS:** There was a young girl and her name was...
- VM:** ...Dea Lea Harrison.
- HeS:** Ja. Dea. Ja. Actually for quite some time we had a motor pool together; one week I was picked up by her and one week I picked her up to save some money for the parking lot at the University.
- VM:** Yes I remember that where you lived was quite some distance. You told me earlier you lived on Berryman. You remembered the number even.
- HiS:** 1820.
- VM:** 1820. And so that was half an hour to walk or so, and so you had a car pool with Dea. What do you remember of the social life at the time?
- HeS:** Actually I was very impressed how the newcomers to this group were taken up by Calvin. We were impressed; we stayed with him, as I mentioned already, and he took his time to find an apartment for us and also all his co-workers were very open and friendly to help us to start the work there.
- VM:** This was not something you had experienced — this was not the German style to do things?
- HeS:** You see in '56, I had no experience in Germany. At this time we didn't have foreigners coming to our labs.; that was due to the special situation after the war.
- VM:** What was the relationship with people at your level and the Professor in Germany, before you went to America?
- HeS:** Of course, there were more formal things. My Professor in Germany never called me by my first name.
- VM:** He didn't?

HeS: No, it was at this time not the case. Later it changed but not at this time. That was a big difference that Calvin called me by my first name.

VM: Immediately?

HeS: I think immediately, yes.

VM: It is interesting that nobody called him by his first name.

HeS: Of course I learned that he was called “Dr. Calvin”.

VM: Yes. It was much later, maybe ten years later, that people began to use his first name — and then only some people. Not the people who had been his students. So Dick Lemmon and Al Bassham took another ten years still before they called him by his first name.

In the lab. itself, how did you interact with the other people working with you?

HeS: I think there was very intense interaction. At first, of course, I had to get acquainted with all the techniques, the places where to find what. I was, for instance, extremely impressed by the fact that carbon-14, which was at this time in Germany extremely expensive, was for us freely available. And on the other hand, in Germany it was not a problem for me to get something done by the glassblower but to get by the glassblower in Berkeley needed, I think, the signing by one of the leading scientists, maybe even Calvin. So there were big differences between Germany and Berkeley.

VM: At the time you first arrived there, how good was your English? Did you understand everything that was said?

HeS: Oh no, we had big problems. In Germany during the war English was not taught. In my school in '43 it was stopped to teach English. So we had big difficulties in understanding the people and also to express ourselves.

VM: But you had learnt some English because I remember when I first knew you I'm sure you spoke English to people in the lab.

HeS: Of course I knew some English but we had especially problems in understanding.

VM: Was it difficult? Did you learn quickly? Because you were by then twenty-something years old.

HeS: Ja, but I don't know. I am sure I am personally not very gifted in learning languages. I think after a few months I was able to go along in the lab. and to discuss with the people and to understand and so on.

VM: I remember one story with your English, which I have never forgotten, and it relates to the book I brought you, that you described the Golden Gate Bridge once as the “So-called” Golden Gate Bridge — and it is an exact translation from German but you cannot say “so-called” Golden Gate Bridge in English because it means it is not really the Golden Gate Bridge it is really something else. That's all I remember. Otherwise I remember your English, as far as I know, was perfectly understandable and there was no difficulty talking with you.

HeS: Thank you.

VM: And I am sure it has become much better since as well.

Did you mix in the social life? What did you do in the evenings?

HeS: One problem especially with respect to learning English was, of course, there were many German-speaking people there like Kanders, like Metzners and so on, and a couple of weeks after we arrived in Berkeley also a friend of us, Achim Trebst, came to Berkeley working with Arnon. And we visited each other also in the lab. but after a couple of weeks, when Calvin met Trebst and I introduced Achim Trebst to him, and a day later Calvin told me he would not like that co-workers of Arnon would stay too often in his lab.

VM: Did he say why?

HeS: No, he gave no reasoning for this but there was a clear statement.

VM: I have never entirely found the reason for this problem between Calvin and (Arnon).

HeS: These people were working on so different aspects of photosynthesis so I didn't understand it either. Nevertheless, it was a fact.

VM: So Trebst did not come to Calvin's lab.?

HeS: Of course, we changed the things, ja.

VM: Did you go to Arnon's lab.?

HeS: I was several times in Arnon's lab. and I never heard from Trebst and I met Arnon there. I never heard that Arnon did say something similar to Trebst.

VM: I think Calvin, for some reason which I say I have not yet discovered, was very sensitive and I may have some possibility of finding out but I haven't done so yet.

VM: So you spent much of your social time with other German-speaking people?

HeS: Especially during the weekends, of course, we made excursions to Yosemite and other places and usually we did it with Germans since they didn't know these places and so on, and they were interested to see them and that was the reason.

VM: Metzners had no car, I remember. I don't know whether Kandler had a car.

HeS: Kandler had a car, ja.

VM: So you used to go out collectively for the weekend with these people?

HeS: That was usually the case, ja.

VM: What about during the week? Did you work late in the lab.?

HeS: Sometimes I returned to the lab. after going home, having dinner and I drove back to the lab. There was also no problem to find a parking place anymore in the evening at seven o'clock or so and that was the case, ja.

- VM:** Did you make any trips to other places, apart from the local trips around California? Did you go further than that anywhere?
- HeS:** During this time, during Christmas, there was a big tour together with Kandler, with Trebst, Provis (*spelling?*) (Provis was a scientist from France working in Arnon's lab.), and Chris van Sumere. We had two cars and we were travelling, I think, roughly a fortnight to New Mexico, Death Valley, Grand Canyon, and so on.
- VM:** So you saw all the things that people ought to see when they go to the West Coast?
- HeS:** That's what we did, ja.
- VM:** Did you find the style of living of the Americans very different from what you had been used to?
- HeS:** Of course, it was extremely different. It was such an easy way of living. Usually when I was asked what are the main differences between old Germany and the States, maybe I heard it or I don't know whether I invented it, but I answered usually the most different thing is the following: in the States one is shopping only once per week and taking a shower every day. In Germany one takes a bath once a week and goes shopping every day. (*Laughter*)
- VM:** That's very nice, yes. I see what you mean.
- Hildegard, you have been very quiet so far. Can you tell us something about what your life was like in Berkeley in that time?
- HiS:** Ja. My English was much poorer than Helmut's. He had acquaintances in Colusa, California, only by letters, and Mrs. Davidson came to see me and asked if I would need something and so. I thought I would like to work a little bit. Then she told me she had a lady, a secretary somewhere at the University, she has two children, a pair of twins, not married, and if I could take care of the twins? So she introduced us and then I worked in the house, in the apartment of Dell (*spelling?*). The twins were a boy and a girl about 2 years or 2½. I came in the morning and I made lunch for the children and I put them in bed after lunch and I walked with them when the weather was nice and I talked with them and I made mistakes because I said, for instance, "this is a cock" and in America it's "rooster"; something like that. (*Laughter*) When they were 3 years old (she had not much money and so she paid not much); then, when he was 3, they put them — she wanted that they go to the kindergarten. Then I had to go in the morning to pick them up and bring them in the kindergarten which was a few houses beside ours and I picked them up...I don't know: noon or later, brought them home and stayed until the evening Dell got home.
- VM:** Did you drive?
- HiS:** I didn't drive. I learned it...but the kindergarten was close and the apartment of Dell was also very close; I have forgotten the name of the street. This was what I did and I had magazines to complete my English and things like that.
- VM:** Did your time in America change your style of life when you came back to Germany.
- HiS:** What we did before, we had lunch every noon and then Helmut said "it is not so good, you are so full after lunch" and then in Berlin he came home in the evening but he didn't come home for lunch and I cooked in the evening.

VM: That was different from the normal German style?

HiS: This was different and was also very different for me because we at home we always had a warm lunch at noon and in the evening we had tea, bread, cheese, sausage, like that. Then we changed this and only when Helmut was in Weihenstefan and he came for lunch home. But in the evenings we also had a warm meal because he likes it.

VM: These are significant factors in your life.

HiS: Other things didn't change.

VM: And you worked with these children for most of the time you were in Berkeley?

HiS: I don't know when it started?

HeS: About nine months of the year.

VM: I remember that you came, of course, to all the lab. parties that we had there — Christmas parties and when people went away we had parties. Did you have a party when you left Berkeley?

HeS: Yes, there was a farewell party.

VM: And that was in the summer of 1957?

HeS: Ja. At the end of June, or so.

VM: How did you travel back?

HeS: Going back was an interesting thing. We bought a car right after we arrived in Berkeley for \$80 and since we observed that it used a lot of oil I went to the widow — actually the widow asked me how I am satisfied with the car and I told her it uses too much oil so she gave me \$5 back. So we had the car for \$75 and we used this car during the year. And then we drove back from Berkeley in a zigzag across until to Allegheny State Park in the East. The last day from the Allegheny State Park we had the idea to go to New York again and the car was not to start any more. So we had to go to the bus. But nevertheless we had, I think, about 15,000-20,000 miles on this old car.

VM: Not bad for \$75 plus a lot of oil.

HeS: But, of course, I learned then. In the lab. there were rather a lot of people, experts on old cars, gave me such a tank, an empty tank, and told me I should buy old oil, that would be good enough. So I had always in the trunk maybe five gallons of oil.

VM: Yes. So when you finally went from New York did you go back again by sea to Germany?

HeS: Ja.

VM: And you came back to the place where you had been working before in Berlin.

HeS: In Berlin, that's correct.

VM: Briefly, what happened in the rest of your career?

HeS: In Germany?

VM: In Germany, yes.

HeS: Ja. I was surprised when I came back to Berlin. Weygand, my boss, told me he will very probably leave Berlin. He had at this time three offers, one to Hamburg, to Bonn and to Munich. He selected Munich and from Berlin in spring '59 we moved to Munich and in '58 I had my *Habilitation* in Berlin and then I left Berlin and came to Munich. In '64, then, there were to do some decisions. In this year I got the offer from Arnon as Associate Professor in the Unit for Plant Physiology.

VM: In Berkeley?

HeS: In Berkeley. And another offer was from the University of Lausanne in Switzerland and a third one from the Technical University at the Agricultural Faculty. There was a chair for chemistry. So, after some negotiations, I selected the last offer and there I stayed for seven years and then I got a call back to the Faculty of Science, in '71, of the Technical University in Munich and there I stayed until the rest of my time.

VM: In your earlier period in Munich you said the University of Munich. Is there more than one university in Munich?

HeS: In Munich we have a Technical University and the University of Munich, or the exact name is Ludwig Maximillans Universität.

VM: And which one were you at before?

HeS: I was always at the Technical University.

VM: Did that used to be in the city or was it always in Garching where it is now?

HeS: No, that was in the city; we moved to Garching, I think in '77.

VM: And so you also moved to Freising at that time?

HeS: No, we moved to Freising when I had the position as a Professor of Chemistry on the Faculty of Agriculture at (*indecipherable*) which is in Weihenstefan and Weihenstefan is a part of Freising.

HiS: That was in '66.

HeS: That was in '66.

VM: There is something I am not clear about: since you know the American system you can probably make the connection. What is *Habilitant*, approximately, in the American scale of ranks?

HeS: It is a difficulty to compare. In Germany if you want to make a career at the university, and you are not an engineer or something, then you have to do it via

industry to get a certain position in industry and then you may come back to university. If you don't do this, and there is some kind of an exception, at the university about four to eight or ten years after your Ph.D. studies, you have to submit another kind of work which we call *Habilitation*.

VM: That's research work?

HeS: That's research work. And then after you fulfil all the things supplying this and taking examinations and so on you are allowed to give lectures. Then you are *Dozent* and then you are able to get offers on professorships from other universities.

VM: And during the period of *Habilitation* how do you get paid? You have a salary from the university?

HeS: Of course, *Habilitation* doesn't mean a position so what you need is a laboratory where you can find a place and facilities to work with. Besides this, a position — scientific assistant or so — and you have to fulfil, of course, duties and when those things come together then you have a chance to reach your *Habilitation*.

VM: During this period you do only research?

HeS: Not only research since you have to have a position for which you are paid. You are not paid for doing research.

VM: So what do you get paid for doing?

HeS: For instance, as a teaching assistant or so.

VM: So you can't give lectures.

HeS: No, like courses and things like that — seminars.

VM: Is that still the system in Germany?

HeS: That is still the system in Germany.

VM: I think the last thing I would like to ask you — but there may be more things that you can think of — the last thing is the building in Berkeley, the Old Radiation Lab., the wooden building you worked in. Many people think that was an important factor in the way the lab. worked because everybody was thrown together in one place. What is your view of that?

HeS: Ja, I think that was really an important thing, this Old Radiation Lab. Every newcomer was integrated very fast and I remember the mornings at ten o'clock the coffee round on this big table in the centre of the lab. and I think it was well known to set out with the idea to reconstruct, so to say, in the later building the so-called Calvin Circus.

VM: It was the idea. Do you think it looked successful in the new building?

HeS: Of course, I was only one or two times briefly in the new building but I think one tried, as far as one can do it from the building construction, to keep this idea that people come together, that people cooperate, and I think it worked but you should judge.

VM: How did that sort of interaction compare with your other experiences before you went to California and later during the rest of your career? Do you find German labs. like that?

HeS: By the experience which I got especially in Calvin's group, I was influenced to try it whenever it was possible to use some of these ideas in Germany. In Germany we have the tendency to put borders between things and in organic chemistry, organic chemistry, own institutes and so on, and I think there should be no borders and there should be cooperation between physical chemists, biologists, geneticists, botanists and so on and that this cooperation, this can be successful I learned in Berkeley and that was a long-lasting sense and maybe a very important experience which I got in Berkeley.

VM: In your own group, later in your career when you were a professor, did you have such an open arrangement yourself?

HeS: I was always interested to work somewhere on borderlines and in my group, not always at the same time, but, of course, there were chemists, a physicist and usually I had always one or two microbiologists in my group and that was, I think from this aspect, at least fruitful.

VM: Did you also have foreign visitors here?

HeS: Of course. I had during the years many foreign visitors. Actually most of them came from Japan. For a series of years from the '70s until the end of the '80s I had a series of Japanese post-docs. but also Japanese companies sent scientists, young scientists, with their families to Germany. The man can work in my lab. and often these people were not chemists; they were, for instance, were again microbiologists called them biotechnologists.

Tape turned over

VM: It seems like your experience in Berkeley, from the organisational point of view, had a long-lasting effect on your own thinking about it.

HeS: That was definitely the case. I was also impressed by this Friday morning seminar and to hear things from so many different scientists in respect to science they are representing. I thought these things were started rather late in Germany and sometimes they are today not started actually.

VM: So did you run seminars in your own group when you had a group?

HeS: Of course, yes.

VM: In the same style of easy, open seminars?

HeS: Ja. We did this in a similar way and, of course, also our guests usually they came invited to give a seminar just to tell in which fields they are experienced and they want to learn and so on.

VM: You must have found it difficult to give a seminar in Calvin's group because Calvin was always interrupting people. Was that difficult for you?

HeS: I think I was talking twice if I am not mistaken. Once Calvin asked me to talk a little bit about my work in Germany and also, based on this, he wrote to a series of laboratories to which I was invited to give seminars on our way back from Berkeley to New York. As I mentioned already, we were up on the Eire Sea and down to Oklahoma so it was on a zigzag like...

VM: Down on which sea did you say?

HeS: The Eire Sea.

VM: Oh, Lake Eire.

HeS: ...and once I gave a seminar on work which I did in Calvin's lab.

VM: I am very glad that you remembered so much and I am very interested to hear that the time you spent with Calvin has been reflected in your own career as you went through. I think many people who spent time in that lab. were very impressed with the way it was organised, particularly the enthusiasm which, for Europeans at that time, was a very unusual experience to meet that. I guess, like the rest of us, you enjoyed your time in Berkeley.

HeS: Definitely. Actually, at the end of my stay Calvin offered me a grant for another year — it was about two fold the amount of money I got from Fulbright. I asked my boss in Berlin and he answered me if I stay a second year he cannot guarantee my position in Germany so that was the reason that we returned after one year. Actually, we would have enjoyed it to have stayed a second year there.

VM: But you were not, clearly, ready to risk your position in Germany.

HeS: That is right, ja.

VM: Had you ever thought of emigrating to America?

HeS: Definitely, when there was this offer from Arnon's lab. but actually they wanted a definite decision, yes or no, after a fortnight. That was too fast for us. When I got this letter from Arnon and of course the first days we thought about it and talked to our parents and then after a week we started to talk to a company what may it cost to deliver your equipment and furniture from Germany to Berkeley and, after a fortnight, I got a letter astonished I didn't answer yet. Of course, I told them that I received this letter and so on but I didn't tell them my decision. And after a fortnight they wanted a decision. And after another week we decided that is too fast so I decided to stay in Germany.

VM: But if they had been more patient, if they had given you another few weeks, might you have gone?

HeS: The probability is rather high, I would say, more than 50%. Berkeley was still a place which we liked extremely and we had such an interesting time there so the chances were high to...and also the scientific living on the campus, that was so impressive. But we couldn't decide in a fortnight!

VM: I understand, yes. I think I have asked you enough questions and thank you very much for everything you have told me.

HeS: You're welcome. I am really looking forward to what your book will look like.

VM: So am I. Thank you.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Helmut SIMON

Date of birth April 14, 1927 Birthplace Würzburg, Germany

Father's full name Georg SIMON

Occupation employee at private industry Birthplace Wüstviel, Germany

Mother's full name Emma SIMON, born Kunz

Occupation house wife and mother Birthplace Ludwigshafen, Germany

Your spouse Hildegard SIMON, born SIMON

Occupation mercantile employee, housewife Birthplace Ludwigshafen, Germany

Your children Karl Michael SIMON

Where did you grow up? Würzburg until 1930, Ludwigshafen

Present community D-85354 FREISING

Education studies of chemistry at the universities of Mainz and Heidelberg, Ph.D. thesis univ. Heidelberg 1954, Habilitation TU Berlin 1958

Occupation(s) Scientific assistant 1952-65 at the universities of Heidelberg, Tübingen, TU Berlin, univ. Berkeley, TU Munich, 1965 full prof. TU Munich

Areas of expertise isotope methodology, reaction mechanisms in organic chemistry and biochemistry, redox enzymology, esp. in microbial anaerobes, and its preparative application

Other interests or activities biotechnology, history of science

Organizations in which you are active American Chemical Soc.,
Ges. Deutscher Chemiker, Ges. Biochemie und Molekularbiologie
etc.

Chapter 42

OTTO KANDLER (with Traudl Kandler)

Freising, Bavaria

March 15th, 1997

VM = Vivian Moses; OK = Otto Kandler; TK = Traudl Kandler

(Editorial note: parts of this recording were unclear or incomprehensible. With the respondent's agreement, the transcript has been edited to alter these sections and so does not correspond exactly with the recording; the edited sections appear in italics.)

VM: This is a conversation with Otto and Traudl Kandler in Freising on the 15th of March, 1997.

I remember that you were in Berkeley in 1956-1957 and you came in the middle of the year.

OH: In September, I guess.

VM: How did you come there? What brought you to Berkeley?

OK: I had an offer from the Rockefeller Foundation to spend a year in the States, I think it was in '53 when they offered it to me. In those days I worked in photosynthesis and I had some doubts about the correctness of Calvin's scheme which was not perfect in those days anyhow. *I did not like the idea that photosynthesis is a reversal of glycolysis because it involves so much ATP and it requires that the distribution of C^{14} in hexose is symmetrical.* This was not definitely shown in those days because the degradation was done with *Lactobacillus casei* which gives symmetry anyhow. Since I learned my biochemistry from Feodor Lynen I also used his method to determine the phosphorylation rate which was stopping metabolism by cyanide to measure the increase of inorganic phosphate which was supposed to be more or less equal to the *phosphorylation rate* a few seconds before. I transferred this *method* to *Chlorella* because the Chemical Institute was bombed *during the war*. Lynen worked in the Botanical Institute.

VM: In Munich?

OK: In Munich.

VM: And you worked with him there?

OK: I worked not with him but next door.

TK: Because he had no institute; his institute was ruined.

OK: And Holzer was one of his assistants. I got good friendship with Holzer and I always looked at what he was doing. Then I heard in a lecture by Lynen how he made *phosphorylation kinetics*: every two seconds he took a sample. I was very impressed by this. When I saw his assistant *Holzer doing such experiments, just shaking his Erlenmeyer flask with the yeast suspension, taking out samples every few seconds and putting them in trichloroacetic acid*, I said this is not difficult; I can do the same thing with *Chlorella*.

VM: Probably not accurate, either.

OK: I did the same with *Chlorella* and this is why I came to this technique. I found out that inorganic phosphate pool goes up and down with light on and switching off the light, in *Chlorella*. I said this was a sign of light phosphorylation. I did this already, when was it? In '47.

VM: Long before Massini did it in Calvin's lab.

OK: Yes, long before. I published a paper in 1950. *At first* I just measured these changes in the inorganic phosphate level *during the light/dark and the dark/light transients* and then I used the cyanide technique. I was a little disappointed because I thought I should pick up a much stronger increase of inorganic phosphate *in the dark* with cyanide. It actually wasn't more than I got with *Chlorella* after glucose feeding. *If one calculates the phosphorylation rate on the basis of a stoichiometric coupling of photosynthesis with phosphorylation, one would expect a 3-4 fold higher phosphorylation rate during light saturation than in the dark under saturated glucose feeding. So I wondered why I could not pick up the expected much higher phosphorylation rates in the light. On the other, I was convinced that a light-driven phosphorylation exists, as evidenced by enhanced glucose uptake in the light.* It's converted to sucrose and other things. And so I thought, well, this glucose uptake is also a measure of photophosphorylation. The rate I calculated from the glucose uptake and from the change after cyanide stopping was about the same. And so I concluded that light phosphorylation is *a reality* but it does not support the full photosynthesis according to the Calvin cycle. If you use two ATP for each CO₂, and you need an extra one to convert glucose to starch; *the phosphorylation rate in the light* should be much higher.

VM: Than you could generate by light phosphorylation?

OK: Than you could generate by light phosphorylation, yes. Then in '54, I think it was, Martin Gibbs came to Munich and gave a talk in Lynen's seminar and told about his *Leuconostoc* study. I thought this is the method to use, to look if Calvin is right, that the *distribution of label in the freshly-synthesised glucose* is symmetric. I decided to spend half a year with Martin Gibbs to learn this technique and then to go to Calvin and to do experiments with P³² which was difficult to do in our country because it was still — we didn't have a military government any more but it was still complicated to get permission to work with isotopes.

VM: At that time, what was your position here in Germany?

- OK: I was an assistant at the university. Each professor in our country in the older days had a couple of assistants.
- VM: Who was your professor?
- OK: My professor was in those days (you don't know him) was Leo Brauner...
- TK: A botanist?
- OK: Yes, a botanist. In the early days I didn't have *a full professor of botany* when I was a student. But then it was Leo Brauner and then it was Otto Renner.
- VM: What was your own background? What were your academic studies before you did research?
- TK: I'm sorry. It was Renner first and then Brauner.
- OK: It was Renner first and then Brauner, yes. I took botany and chemistry for examination for the doctor thesis. I took physics because Gerlach, a physicist, gave an excellent lecture and so I went to him. My main training was talking with Holzer, the assistant of Feodor Lynen, and watching him.
- TK: You didn't have a teacher, actually, because the Institute because after the war there were no professors.
- OK: After the war there was no professor *left* because they were all by the Nazi party.
- TK: And were all removed. We had not...
- OK: The Nazis were actually Green/Red; it was a combination of Greens and Socialists. They believed in anthroposophy and so on, and *many were* vegetarian. I only realised this nowadays, how this really worked. Biology was a big thing during the Third Reich.
- VM: So it was easy for you to study biology? That was a...
- OK: But we had nobody. Yes, there were three *young ladies who taught us elementary botany*. We also invited professors *such as Bünning and Egle* from other universities. I don't know whether you know Egle; he worked in photosynthesis after the war. But Purson, you may...
- VM: Purson? Where? Is he here? Is he German?
- OK: He is German, yes.
- VM: No, I think this is before my time.
- OK: Yes, this is before your time,
- VM: I was never a botanist, I was a microbiologist, biochemist. It's interesting because many of the people who worked at the beginning in photosynthesis were all chemists, particularly the Americans. All the Berkeley photosynthesis people were really chemists. Only Van Niel — he was probably a microbiologist.

OK: A microbiologist; yes, he was a very famous microbiologist.

VM: So you planned to spend half a year in each of the two places?

OK: Yes, because I wanted to learn at first the breakdown of glucose, carbon by carbon, just to be sure that it really was symmetric; or if it's not symmetric. It was almost symmetric but not quite. It's complicated, you know the story, probably. Then I went to Calvin to do the P^{32} experiment. *I expected a very strong uptake and fast labelling of phosphorylated sugars but it was not as strong as I expected* from my cyanide work. When I arrived, I realised that Bradley had already finished his thesis but I didn't know that he worked on *phosphorylation*. Calvin didn't tell it to me. I talked with Calvin in Brussels at the Biochemical Congress in '54.

VM: Was that when you first met Calvin?

OK: Yes, I met him first in Brussels, I think it was in '54 or '55.

VM: He was in Europe in that summer of 1955. You arranged with him then to come to Berkeley.

TK: The International Biochemical Congress.

OK: Yes, the International Biochemical Congress in Brussels; I was there, too.

VM: And you arranged with him that that you would...

OK: I arranged with him then.

VM: Did he know about your own work?

OK: I think he didn't recognise it. I told him about it but I think even later on he didn't recognise it, really, because he never cited it.

VM: Did he encourage you to come when you said you wanted to come?

OK: Yes. He was always happy to have someone come if it didn't cost any money for him. He was very friendly of course. I talked in Brussels about this phosphorylation business but I think he didn't get it to his mind what it means.

VM: I think there were times when he did not allow things to get to his mind that he didn't want to hear.

OK: He was not very happy when I talked in Berkeley about it. Only then when I arrived in Berkeley I realised that Bradley had done exactly the experiments I had wanted to try. I was very satisfied reading his thesis and looking at (*his results*). It showed there is not very strong, not very fast phosphorylation in the light — the *labelling of ATP* was very sluggish,. Of course, Calvin in the discussion always said, "Well, you know, there is a compartmentation in the cell and it just doesn't get into the chloroplast. This is probably the explanation why the exchange, the labelling, is so sluggish".

TK: It was never published, didn't you say?

OK: It was not really very well published (the Bradley paper). It was in the local...

- VM: UCRL (*University of California Radiation Laboratory*) report.
- VM: It wasn't published as paper? I just don't remember.
- OK: I don't think so. At least I didn't see it. I read it in the report and in the original dissertation.
- VM: I don't have a list of Bradley's papers but I have a list of Calvin's papers. Probably, if it had been published, he would have published it with Calvin. I will look it up when I get home to see whether it's there.
- OK: There is one Calvin and Bradley (*paper*), but it's not very detailed on this subject. It's clear because it was difficult for him to explain it. He had to argue. If you like to push something, you don't like to argue too much! It looks as if you didn't know it really.
- VM: So you arrived in Berkeley in September 1956, intending to stay for about six months? That was your plan?
- OK: I think I never wrote him. I just talked with him in Brussels and all the other things were handled by (*Gerard*) Pomerat; this was the Fellowship Adviser (*of The Rockefeller Foundation*). I don't remember that I wrote a letter to him.
- VM: The only thing I wonder is that in those days everybody had to have clearance to work in that lab. Foreign citizens were more of a problem. I remember I had to get clearance. I presume you got clearance somehow.
- OK: I think Pomerat took care of this.
- VM: You arrived one day and Calvin was there when you arrived?
- OK: Yes. They had arranged a house for us; we lived on McKee Road...
- TK: McKee Avenue.
- OK: McKee Avenue.
- VM: McKee something, anyway. Americans don't worry too much the "avenue" or the "road". You bought a car, did you, when you got there?
- OK: We came by car. We bought a car in Brookhaven. Because after three days we found out that Brookhaven is... even buy a can of milk or something you had to have a car! We bought a car for \$80.
- VM: And you drove it all the way?
- OK: Yes and I sold it for \$100 when we left.
- VM: Congratulations! Very good! OK, so there you are in Berkeley on the first day or the second day and you have a house on McKee and a car.
- OK: We drove up to Calvin the next day and we got stuck because our car was too weak to make it up the steep hill.

TK: I think we had to walk the last part!

VM: I remember that you moved into the Old Radiation Lab. with the rest of us.

OK: Yes, that's right.

VM: I remember you spent a lot of time with Calvin in that office which had glass walls, but I don't remember where you were working, which lab bench you had.

OK: Together with (*Helmut*) Simon, I shared a bench with Simon. They slept, they spent the night...

TK: They spent the night...(*Do you know*) the story of how we met? We didn't know each other.

VM: No. Come on; tell me. They told me they came late and they had to a night in a hotel in San Francisco.

TK: We had been in Berkeley for two or three days and the Calvins invited us for dinner. We came there late in the afternoon, five or six, and Hildegard and Helmut Simon were already there because they had spent the night there. Somehow, they didn't dare to leave the room. They noticed that the Calvins had some visitors; they didn't know who it was. After a while they thought "We must go out, it's getting late" and this was us. They saw us for the first time.

VM: Did you know they were going to be there?

TK: We have been very good friends ever since that evening.

OK: In those days, they were in Berlin with Weygand, they were not in Munich. Only afterwards, Professor Weygand moved to Munich and they moved with him to Munich.

VM: Yes; Helmut told this morning his story. So anyway, I'm placing you in the lab. and so you're starting to work.

OK: And then, of course, I did CO₂ fixation because I wanted to see what cyanide does with photosynthesis. This was the main purpose. I also knew that Meyerhof showed that triose is an intermediate by stopping with cyanide and with *hydroxylamine*. I thought I could do the same thing with photosynthesis because *dihydroxyacetone phosphate* was tricky in those days. You only had a very faint spot of triose phosphate so the point was: is it really there? I thought it should accumulate if you put cyanide in. I was surprised when it didn't accumulate but the hamamelonic acid accumulated. So you know that story about the hamamelonic acid with...

VM: I remember there was a story. That was an addition product, presumably; a cyanide addition product.

OK: Yes, it was of ribulose diphosphate. When I came back to Munich, Helmut prepared some labelled cyanide and we did the same experiment as Rabin did in Calvin's lab. I came to the same conclusion. There was some tricky thing. We *degraded the isolated hamamelonic acid which should have been labelled only in the carboxyl group* but we didn't find it. It was dispersed. That may not have been very clear by the degradation. But I *accepted* that it was a cyanide *artefact*.

VM: Was Calvin very interested in what you were doing? Did he talk to you a lot at the beginning?

OK: Not too much. He became very interested when I had this unknown spot; I think that was the first time that he put the keto acid into the scheme was a few months later. Now he saw that it could be trapped. But he didn't talk too much with me.

I then came back to Munich to work on the hamamelose. *hamamelose is found in almost all plants, especially in Primulaceae*; they have lots of hamamelose — it's the main sugar. I still wonder where it comes from. Probably it is made from fructose diphosphate because Bick showed in my lab. that aldolase can manage to make hamamelose from it.

VM: There was a period in Berkeley, I remember, when you used to argue a lot with Calvin and you were in that office with the glass wall and we could see you on the blackboard, the two of you, arguing. What were you arguing about?

OK: How he explains this very sluggish phosphorylation. If it's really necessary to have so much phosphate. I always thought it *would be* stupid to *build* something, drag it down half way and then spend again so much energy to bring it up. Even today nobody has shown, to my knowledge, that the turnover of phosphate really has this high rate. You can show it very easily in isolated chloroplasts, making phosphorylation with chloroplasts. You get high rates, you get 300-400 micromoles, but not in whole cells. But as Calvin argued, the *sluggish* exchange *between chloroplasts and cytoplasm may prevent its detection*.

VM: You weren't satisfied with that explanation?

OK: I was not satisfied with that explanation and I spent a lot of my time, I wasted some of my time, to find the trick to show the other way. But it didn't work. I also did much unpublished work about the exchange. If you feed labelled glucose, position-labelled glucose, and look how fast it equilibrates, all these things indicate that the exchange between chloroplast and cytoplasm is very good. This is why it took me a long time to drop this issue.

VM: When you were there, I'm sure when you were there, the argument grew very intense, I remember, and Calvin was clearly...

TK: I didn't know that.

VM: You didn't know that?

TK: He always told me about his ideas and about the argument but I didn't know that there was such a discussion behind glass doors.

VM: There was a room there which had been Al Bassham's office, I think. Al Bassham was away and I don't know whether you occupied the office but when Calvin came by...

OK: No, I didn't occupy the office.

VM: ...they used to go in this office and shut the door and you could see, and on the blackboard, but we were on the outside and we could see but we couldn't hear or we

couldn't hear clearly. Obviously, we understood what the nature of the discussion was. I remember that we then had a meeting of everybody in the building really to discuss this issue. Do you remember that?

OK: Yes.

VM: As far as I remember, we resolved it. What did you feel, in the end, about the whole business? Do you think that there really was a discrepancy between your results and his results? Were you happy in the end with the cycle? What was your feeling?

OK: I still have some doubts. You know the paper of (*Elias*) Greenbaum, probably, from Oak Ridge?

VM: No. I left this field, I have to tell you, in 1958. I have not been involved with these things since then.

OK: The Greenbaum paper is exactly what I feel. He has a *Chlamydomonas* dominance mutant (*Editor: correct?*) which has no photosynthesis system I. Of course, this was after the Calvin time already. I always thought that there was some special system for the phosphorylation and I associated always the Photosystem I with phosphorylation and Photosystem II with real CO₂ reduction. He has a *Chlamydomonas* dominance which has no Photosystem I and still it makes *oxigenic* photosynthesis and the quantum yield is very good. He says that Arnon was right, there is no Z scheme, no obligate. You can *do* it with Photosystem II alone.

VM: What does this mean for the path of carbon?

OK: He (*Greenbaum*) still accepts the Calvin cycle. It wouldn't necessarily change the path of carbon. I am not quite *sure what he means*. He does need less *quanta*; he gets a better quantum yield. But if you split the two PGA and have to reduce both, you spend a lot of energy so the quantum yield shouldn't get much lower. I don't actually see what he feels about the Calvin cycle. In his paper there is no evidence that it is Calvin cycle.

VM: So you still have some reservations about some parts of the Calvin cycle?

OK: Yes. There may be still some way that you don't need so much phosphate.

VM: Does anybody still work on the path of carbon in photosynthesis? I would have thought it's long dead, isn't it?

OK: I don't think so. The only point where one could start is this mutant now of Greenbaum. He actually should do the old Calvin-type experiment.

VM: Nobody has the equipment any more...I will tell you later about the equipment.

TK: It's not very difficult.

OK: It's not too difficult. You can set up a chromatogram very quickly. I could even do it as an emeritus now. (*Laughter*) It would be difficult today because of the new laws to work with isotopes.

VM: Is it difficult?

OK: Yes, it's difficult. It's even difficult to work with chemicals.

VM: Anyhow — to take you back 40 years because modern history, I'm not doing: I'm doing old history! You worked on the hamamelonic acid afterwards and that's what you did in Berkeley?

OK: In Berkeley I made a lot of fixations and chromatograms. Essentially I tried to catch the keto acid and used hydroxylamine and other possible things under a variety of extraction conditions. I used the cyanide and the *various kinds* of application of cyanide.

VM: Did you find it? Do you think you found it?

OK: I trapped successfully the keto acid, of course, in the form of hamamelonic acid. But Helmut (*Simon*) told me right away, that it could have been a cyanohydrin synthesis. I am not a well-trained chemist but fortunately Helmut who had the experience and was a very good chemist and helped me in this respect.

VM: Did you publish that with Calvin?

OK: No. I published a paper on the finding of this unknown. He helped me to write it but not with his name.

VM: Looking back on that lab as you try and remember it forty years ago: was it a very different type of environment from the one you had previously known in Germany?

OK: It was different, yes, of course. I had two labs. there and I was very fortunate that Lynen was in the Botanical Institute so it was not "pure" botanical institute...

TK: Traditional.

OK: Holzer's work was not so much different than the work of the people in the Calvin lab.

VM: You had already experienced this very free atmosphere of talking between people in the lab. already before you went there?

OK: It was not unfree *in* Germany.

TK: After the war.

OK: After the war I think the people in the lab. had no restrictions.

VM: No, but there was an atmosphere in Berkeley, at least many of us who came from Europe experienced this, the easy relationship the Americans have with one another.

TK: We had experienced this already in Brookhaven.

OK: This is not specific for the scientific world. It was a general way. For example, the rich man talks with the person in the filling station. This was different in Germany than in America. Also the connection between the students and professors was, of course, quite different.

- TK:** Much more casual (*in America*): coffee breaks were very important and very new to us when we arrived in Brookhaven National Laboratory. We had had this experience already there. Coffee breaks were where people got together and discussed their daily work. So this was very new.
- OK:** In our institutes, in those days, there weren't as many people. Put it this way: my connections, for instance, with Holzer (*in Germany*) was not much different than the connection to...
- TK:** It was a very small group.
- OK:** Well, of course, he was the only assistant of Lynen; there was only one. It was much simpler.
- TK:** The standard of living was very different, of course.
- VM:** That was for all of us from Europe in various ways.
- TK:** When did you arrive?
- VM:** When you did. I think two weeks later or something.
- OK:** The exchange in the laboratory was not so much different but it was a very stimulating atmosphere, of course.
- VM:** The way you describe some of your interactions with Calvin suggests that Calvin was, of course, very concerned that his ideas should be accepted by the scientific world.
- OK:** Of course; that's natural.
- VM:** Did you find him an original thinker?
- OK:** Of course, yes. He was certainly an original thinker.
- VM:** I recently wrote an obituary for him in which I stressed that so many of us thought he was a great scientist. Do you think he was a great scientist?
- OK:** I think so. I don't know the other things he did. Later on his ideas about the petroleum plants, I think this was a little trivial, he had not so much physiological experience with plants.
- TK:** As scientists grow older they try to do something which has more practical relevance and which is more understood by the public. Remember your own activity with "Waldsterben" (*death of forests*). We have this problem in Germany where some pseudoscientists claim that we have a general dying of our forests and Otto thought this was wrong and he also tried to change the discussion. This was also trivial; it was not solid science, actually.
- VM:** When Calvin was involved with the petroleum plants, he was already in his seventies, he was no longer director of the lab., he didn't have the responsibility and, of course, the atmosphere, after the photosynthesis, was never the same. During the early days there was a strong focus, you know, and everybody in the building worked toward the same objective. Then it was finished; people did other things.

What did you think of the building? People have talked with happy memories of that wooden building. Did you have happy memories?

OK: Yes, of course.

VM: Do you think it was a good building for doing science?

OK: It was a good building because especially they didn't care very much about the regulations. This is always nice to work with.

VM: Nobody died, as far as I know.

OK: And we still have our hair.

VM: Some of us!

OK: I didn't lose any!

VM: Did you ever go back to Berkeley to see the new building?

OK: Yes. I wouldn't like to work there.

VM: Why not?

OK : It looked so...

TK: Sterile?

OK: It looked so sterile, this large room; maybe it's different if you really worked there.

TK: Maybe it's just nostalgia.

OK: If you had not worked in the old building, it had a look...if today you would come back without the experience, you would say it would be impossible to work there!

VM: In the old building?

OK: Yes. I wouldn't be happy in this circus...

VM: ...the Calvin Circus.

OK: The Calvin Circus, yes. I visited once Bassham there.

TK: And Arnon.

OK: But Arnon was not there.

VM: Arnon was not in that building.

OK: This was a sacrilege.

VM: Do you know why Calvin was so antipathetic to Arnon?

OK Of course; both struggled for a Nobel Prize.

VM: Was Arnon against Calvin like Calvin was against Arnon?

OK: I don't think so, that he was so strong against Calvin personally. Well, it was another personality there; you couldn't judge it so easily what he really feels.

TK: I never heard any negative remark by Arnon about Calvin.

OK: In this respect Arnon was (you understand German) more *vornehm*.

TK: Very reserved.

VM: You think even then, in the mid-fifties, Calvin and Arnon were both thinking strongly of Nobel Prizes?

OK: I think so. Arnon thought his photophosphorylation was important. Arnon visited me in '54 (something like that) and he gave a very detailed description of my work in '56 but later on he doesn't mention it any more.

TK: Why didn't you go to Arnon? Why didn't you want to go to Arnon instead of Calvin?

OK: I was interested in the path of carbon; photophosphorylation was an old story for me. I was convinced it works and you can use it for sugar assimilation and all those things, so this was not a problem. It only was a problem together with the Calvin cycle if it really is strong enough. That it was there, that's no question, but the stoichiometry was the important thing and it was not easily shown. Of course, the excuse Calvin uses is legal and is feasible to me, too. But it was just wishful thinking on my side that this exchange is not the right explanation and in some way one should be able to show it. This paper now of Greenbaum is really surprising.

VM: Where is it (*published*)?

OK: I don't have a copy with me. It's in the Proceedings of the National Academy of Science.

VM: Roughly when?

OK: That last one is one year ago (*from March 1997*).

VM: I'll find it. Can we finish by my asking you about your career when you came back to Germany after you had been in Calvin's lab?

OK: I had my assistantship (*in the Botanical Institute*) and I hoped, of course, to get a call for a professorship. I had a call before for microbiology. This didn't work any more because they switched their mind in Köln and they changed — this was what we call an *Extraordinariat*. It was a half-professorship in those days...

VM: In Cologne?

OK: In Cologne, yes, and they decided to make genetics and microbiology and this was not my job. This is why it didn't work. There were some people in Germany who were annoyed with me because I had had an argument with Calvin.

VM: Oh, really? They were annoyed with you?

OK: Yes, yes; very much.

VM: They thought that a young man shouldn't argue with Calvin? Why were they annoyed?

OK: I think Calvin made nasty remarks. I have not heard the remarks but from the echo I got, I had the impression that he...and some of our...you know there is always a certain establishment around the scientists, so this is not the right way...

TK: You didn't behave well!

OK: ...and so I didn't get right away a call for a professorship.

VM: Do you think that had an effect?

OK: Of course it had, a strong effect. In those days the head of the Dairy Science Research Station in Weihenstefan became free and the man who had the directorship came from our Institute 20 years ago, or 25. And so they asked again in the Botanical Institute if somebody was interested. I was interested: in those days botany was a very poor science and I was Dozent in those days, no help at all (a technician), but there were three assistantships in this (*Dairy Science*) Institute, and I knew the predecessor and I was anyhow interested in microbiology. So, I said "Why don't I go there?" So I took the Dairy Science and the botanists, of course, were very surprised that I went into Dairy Science.

VM: This was a professorship in the Dairy Science?

OK: Not really: this was a directorship. So I remained a Dozent at the university in Munich. I had my students; once a week I gave a course in plant physiology because I wanted to have students from natural science and not from agriculture. I should have given talks on agricultural microbiology but this was not obligatory, this was a facultative (*i.e. voluntary*) lecture. My predecessor had only four or five students so I said it doesn't pay to give my lecture for five agricultural students and so I rather preferred to stay at the university and give this course in plant physiology. I had complete freedom in Weihenstefan; I could take natural scientists — I was not forced to have agricultural people in the Institute — and much more money than I would have had in any botanical institute.

VM: Eventually you moved into the university itself?

OK: One year later already — of course, I did dairy microbiology; I got acquainted with lactobacilli and streptococci and in those days I already had started work on cell walls of bacteria, cell wall chemistry and this I could do very nicely. The chemistry of peptidoglycan is very much modified in the various strains so this became a taxonomic characteristic. It was very fortunate that *Leuconostoc* is used to break down the glucose; it also makes the aroma in butter and cheese. So I had no difficulty in making degradation studies with *Leuconostoc* even in the Dairy Institute.

Very soon I got a professorship at the Technical University in Munich. The Botanical Institute at the Technical University was very small and relatively poor so I kept the

Directorship in the Dairy Science which had much more money than the other one. After eight years I got the Chair in Botany at the University of Munich.

VM: Which year was that that you moved to the University of Munich?

OK: In '68. In 1959 I moved into the dairy business (in '58 already) and in '60 I moved to the Technical University and in '68 I moved to the University.

VM: And you stayed there for the rest of your career?

OK: Yes.

VM: Are you now formally retired?

OK: Yes. I retired at the age of 65 in '85.

TK: In order to start work.

VM: Well, that's what happens.

OK: During the time at the Technical University, and also at the University (*of Munich*) I worked mainly on the biosynthesis of branched chain sugars, hamamelose and apiose, and also on other carbohydrates. This was my main interest there. That was one side; on the other side was the variability of oxypeptidoglycan in bacteria and...

TK: Excuse me: and the chemotaxonomy. You worked with oligosaccharides, too?

OK: This was the neighbourhood of the branched chain sugars I worked with these oligosaccharides, mainly raffinose biosynthesis. From then I got more and more interests in phylogeny and evolution, from the cell walls to the archæbacteria. *This led to* my connection with Carl Woese and this is my main interest right now.

VM: Are you still active in the lab yourself?

OK: No, I dropped that. For some years I continued to do a little work on cell walls by myself to screen new bacteria but a couple of my former students have chairs now, they are active and are my assistants now! (*Laughter*) I still have good contacts and talk with them.

VM: It's nice to see you after so long and to see you in such a healthy and active state.

OK: And actually, I'm very happy, after all this CO₂ fixation business. In a few weeks in *Science* there will be a paper on CO₂ fixation under primæval conditions from CO and iron and nickel sulphate.

VM: Are you the author of the paper?

OK: No, just a friend of the author. The author is Gunther Wechteshäuser.

VM: Helmut (*Simon*) was telling me about it.

OK: Yes, he took him in his Institute. I think it's really great. So far the only product is activated acetic acid.

VM: I think we should stop soon because our host (*Editor: this was recorded in the Simons' house while dinner was being prepared*).

I want to thank you very much for spending the time.

OK: It was a great time for me in Berkeley, of course, because to be really in the centre, this was the important thing.

VM: You felt that that was the centre at the time?

OK: For photosynthesis it was the centre. There was Calvin and there was Arnon and I was the only one in the lab. of Calvin who could switch between both! I was the only one who visited every fortnight or so and go down without having an official...

TK: Perhaps that's the reason why Calvin was not so pleased with you. He knew that you had contact with Arnon.

OK: I don't know whether that was so.

VM: I still have to talk with Bob Whatley. Do you know him?

OK: Yes.

VM: Maybe he will be able to help me. No one has really given me a good explanation of why there was so much tension between them. You may be right that it was the Nobel Prize competition.

OK: I think it was just competition.

VM: Anyway; perhaps we should stop and thank you again.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Otto KANDLER
Date of birth 23.10.1920 Birthplace Deggendorf/Germany
Father's full name Karl KANDLER
Occupation Gardener Birthplace Deggendorf/Germany
Mother's full name Theresia KANDLER
Occupation - Birthplace Deggendorf/Germany
Your spouse Dr. Gertraud KANDLER
Occupation Biologist Birthplace Landau/Pfalz/Germany
Your children Maja, Barbara, Susanne Vera.

Where did you grow up? Deggendorf
Present community Munich/Germany
Education Elementary School - High School - University

Occupation(s) Full Professor of Botany (since 1986 Prof. emeritus)

Areas of expertise Metabolism of plants and microbes

Other interests or activities -

Organizations in which you are active University of Munich
Bayer. Akademie der Wissenschaften

Chapter 43

KAREL LOUWRIER

London

April 4th, 1997

VM = Vivian Moses; KL = Karel Louwrier; SM = Sheila Moses

VM: This is a conversation with Karel Louwrier in London on the 4th of April, 1997.

How did you find your way to Calvin's lab. when you first went there?

KL: I was studying in Amsterdam, radiochemistry, and at the time it was necessary to get a degree to work half a year in a non-chemical field and I chose plant physiology. Here I learned about the work of Calvin, where he applied the radioisotopes for his photosynthesis studies, and I proposed for my practical work in plant physiology similar work in Amsterdam with radioactive phosphorus in the transfer of the mechanism of certain sugars. I calculated how much phosphorus I needed and then it was immediately vetoed by my professor who said it was too expensive! Then I made a decision: I'd rather go to America.

I got an introduction from my radiochemistry professor for Calvin and a little bit later I got an offer from Berkeley. They offered me \$400 a month which was, at the time, for me a miracle.

SM: Yes; I remember!

KL: A week later another letter came saying they had made a mistake; they meant \$500, but you have to pay tax. It was reasonable so I loved it and I had to organise my trip. I first applied for Dutch money for the trip. So I came up before one of the premier professors said that what you want to do there you can also do it work in Europe, you can do it in Groningen. I said, "I know, I know, but I don't want to go to Groningen." Well, that killed that.

In the meantime there was another letter from Calvin who said he would pay the trip in the States. Then I found a cheap boat trip on a Dutch boat through the American Field Service; (*one*) could go to the States, round trip for I think it was \$100; a very reasonable price. It was an old troop transporter, basically an old ship that went to the Dutch Indies before the war. I landed in New York and took a train, which was also a new experience.

VM: You were not married at the time?

KL: I was not married. No. I was 26 or 25. Finally, in Oakland, I think it was, at the railway station — or maybe it was Berkeley; anyway, it was in huge...it wasn't a platform, it was just (*indecipherable*) step out of the train, and found a hotel.

VM: Nobody met you at the train?

KL: No, I think I did not tell them exactly when I came. I went up The Hill...yes, I think I went up The Hill to people depending on the Rad. Lab. and there was a very nice secretary who said, "we,, you want to rent a room? She had immediately addresses and then I went to Mrs. Dean in 2581 Etna Street. Mrs. Dean was the mother of General Dean who was captured in the Korean War, I met him also because this was after the war and was released. That is where I stayed within walking distance of the lab., the small, the Old Radiation Lab.

VM: So then you went to there. You had never met Calvin?

KL: I had never met Calvin. So I had to meet Calvin. Of course, I had all my notes but professors were Dutch professors. I specially carried along a huge suitcase with Dutch suit, tie and everything. (*Laughter*) So, when I had my appointment with Calvin I put it all on and then in the lab. he said "you're going to a funeral?". There he was. Like you, but it was a bit more colourful. I was feeling a little bit out of touch, there, and then we had a nice discussion. He asked me what I wanted to do and I said well, basically I wanted to work with carbon-14 and I started working with Al Bassham.

VM: In the Old Radiation Lab.?

KL: In the Old Radiation Lab.

VM: You were in one of those big rooms in the building?

KL: No, it wasn't a big room. I did not have my own office; I had my own corner with a desk. Mainly I was working on the bench. I don't have a clear...I know the place I was sitting and writing. It was mainly measuring and the work with the chromatograms. So I didn't write very much; I was basically working practically.

VM: And you worked with Al?

KL: I worked with Al, yeah.

VM: What did you start to work on?

KL: When I came, I asked Al what he wanted me to do, because it was completely new to me. He said we have these chromatograms with a big spot on it and we don't know what it is; we call it "spot X". Can you figure out what spot X is? I started figuring, well working out, making many, many of these chromatograms.

VM: Was this a carbon spot or a phosphorus spot?

KL: Carbon spot. I worked only with carbon at this time. After some time I found out that is glutamic acid, I think. Al was happy.

VM: Did you say "glutamic acid"?

KL: Glutamic acid.

SM: At which time of the year did you come to Berkeley?

KL: In August. When I came over with this boat, before taking the train I had a few days in New York. For a European it was very warm and a good European I was thirsty. I saw a stand where people were drinking something and I went there and I asked (*for*) a beer. So they gave me something dark. I nearly thought it was poison because it was root beer! You couldn't buy beer on the streets. (*Laughter*)

VM: You didn't make that mistake again?

KL: No, no. In August, of course, it was a lovely time in Berkeley. It was much cooler and I immediately was absorbed in the social life of the group. This was an extremely positive experience for me at that time. There was no time to sit behind and isolate yourself; it was a very active community.

VM: Active, how? When you say you were absorbed into the social life, meaning what?

KL: Going on hikes, Martha Kirk was the motor in this whole thing; I think she was really the mother hen of the group. We went fairly often into the Sierras and Yosemite Park. The glassblower Bill...

VM: Hart?

KL: Was it? Was it called Bill? Bill Hart? There was a rather hefty man but he was in the workshop (*Editor: probably Ralph Norman, the carpenter*) and Bill was rather slender.

VM: I think so; he had a cabin in the Sierras.

KL: He had a cabin, yes.

VM: I think it's Bill Hart.

KL: I spent some time there; it was also very nice. He was a very nice man.

VM: Especially coming from a flat country like Holland, it's very exciting to (*see California*).

KL: Indeed! Absolutely! When I came back, I was shocked — when I came back to the Netherlands; it was so flat, and the houses were that small! The town where I came from — all tiny houses!

VM: This was the first time you had been in America, this visit there?

KL: Yes.

VM: But you had travelled in Europe, presumably?

KL: Not very much. At the time it was difficult to travel around and there were a lot of monetary restrictions but I made, as a student, a bike trip through Belgium and France. That also was an experience because coming from a flat country. My first hills! Actually rocks were completely new at the time.

- VM: Presumably also going to America, then, was your first need to speak English all the time.
- KL: Indeed.
- VM: But you spoke it because you learned it at school, presumably?
- KL: I learned it at school but at school, you get of course the basics and you can read it. During my study, the textbooks that I needed were German and English at the time; very little French. And certain words you never learn at school — or I didn't learn at school: all the things you need for analytical chemistry were new. But anyway, when I came to Berkeley, I had to speak English and the first few weeks this was extremely tiring. I remember that I was in the evening I was so tired and at a certain moment I started to think in English and then it was all right.
- VM: Is English difficult for someone to learn if Dutch is their native language? It's fairly close, isn't it?
- KL: I think to speak it correctly is more...or to write it correctly is more difficult than French. In high school I had difficulty with English to write it and, since I had more years in French — I knew that in general you take a French word and pronounce it differently, you have an English word; it works, not always but...But when I look now at my children, I have two generations of children — my first wife died in '79 and from that marriage I have English-speaking children who are grown up and I have a series of my second wife and the youngest is eight. They watch constantly television, mainly Dutch television, where they have English films. Since we have a computer now and they play all kinds of games, they all play them in English. Their English is developing very fast and they are living in a French community. So they speak French but English comes much easier.
- SM: The problem is that it is not phonetic.
- KL: It is not phonetic, ja.
- VM: Coming back to Berkeley, did you find the...presumably your exposure to English had been mostly with an English accent when you learned it in Holland?
- KL: Yes.
- VM: Was the American accent difficult or did that make no difference?
- KL: That made no difficulties. Of course, it was different but there I didn't have any problem to understand it. During my whole stay in the States, wherever I was, I didn't have a problem to understand their English or their American. I travelled around in Florida and later stayed also some time on the East Coast. On the other hand, in England, in the United Kingdom, there are areas where "my God, what are they saying?" In a bed and breakfast there were some men sitting and talking — it was English; it was not an Arabic language...There are much more local dialects with people...
- VM: What about people understanding you in America, did they have difficulty?
- KL: No, they thought I was English! (*Laughter*)

SM: They are not very linguistically orientated at all.

VM: There were one or two occasions when we had curious difficulties. We found it impossible to convey meaning. They just didn't understand the word we used.

SM: In Oklahoma, they also assumed that since we had a foreign accent it was because we were from California!

VM: We had a California license plate so they presumed that's where we were from. *(Laughter)*

OK — so then you discovered that you spot, that compound "X" was glutamic acid. How long did that take you?

KL: I don't know any more.

VM: Most of the time?

KL: Only a few months. Then I started to work with Ning Pon and somebody else, Rod Park I think.

VM: Was Rod Park there already at that time?

KL: Yes, Rod Park was there already. We started to work with chloroplasts from spinach. To tell you the truth, I don't know exactly what we did any more with that. It was a detail that escaped me. Ning Pon and Rod Park decided, well, you have to make a publication: glutamic acid is not really very much, and they were right. We need to do something else to get a publication. We did part of photosynthesis in the chloroplasts but it escaped me what...

VM: You spent the whole of your period there doing the same sort of work: running chromatograms, doing tracer experiments, counting, identifying — the sort of thing that Al did at the time and other people were also engaged in?

KL: Yes.

VM: So you were very much part of the photosynthesis group activity?

KL: Indeed, yes.

VM: Did you have any contact with the people in Donner Lab., do you remember?

KL: Yes, also because of the outings. I did not work with them but I was contemplating to work with...what was his name? An older chap...

VM: Do you mean Dick Lemmon?

KL: Dick Lemmon, ja. But since I had a restricted period there, I decided to continue with Al. At that time, my real interest was the technique and, I think, the real pathway of carbon in the plants; the basic principles, were already there — it was the details that had to be filled in here and there. In the Donner Lab. they worked with — so Erminio (*Lombardi*) worked there — they worked on completely different issues. It was also

very interesting. I really enjoyed talking with everybody, what they were. And this was also what I liked of this whole group: it was integrated science. In Europe at the time, we didn't have that. It was everybody for himself and there was no contact between groups.

VM: The groups that you had previously known in the Netherlands were presumably much smaller than that...

KL: Oh yes, much smaller.

VM: ...than the Calvin group and presumably, also, they were less well funded than Calvin's.

KL: At the time they were. Calvin had, of course, the brilliant idea to squeeze money out of the Atomic Energy Commission.

VM: He was very successful.

KL: Therefore, I was disappointed at the time because I could not work with phosphorus...

VM: In Holland?

KL: ...in Holland.

VM: Did you ever work with phosphorus in Berkeley?

KL: No. It was not because of the phosphorus itself. If you work with phosphorus, you can count it very easily. In these labs. we didn't have the same facilities; had we had carbon-14, we had to count it the same way there. I think the counters that they counters developed there (*in Berkeley*) were very, very good and appropriate. We didn't have that. I was not really married with phosphorus but the idea to work with an isotope that opens part of the physical science was...

VM: So you spent many hours in that underground room in ORL counting chromatograms?

KL: Yes.

VM: Were you there when the move took place to the Life Sciences Building?

KL: Yes.

VM: Was that a very disruptive period? Did work stop for a long time?

KL: Not for a long time; it stopped for almost a week (?). I think all moves are interesting in the way that you throw away things that (*have been*) staying around for a long time and you don't need it any more. But we had more place. We could build...we had nice racks so it was definitely a good move.

VM: You were in the big room, then, in the Life Sciences Building because the group also had a number of smaller rooms.

KL: The big room, ja.

- VM: So you found the atmosphere fairly similar to ORL in that sense? Many people have felt that ORL was a very significant factor in the way the group worked. Do you think so? The open lab., the informality of it.
- KL: No, I did not. The contacts, the social contacts that I started in ORL continued there. I think we saw Calvin a little bit less in the Life Sciences Building than in the Old radiation Lab.
- VM: But it was further away.
- KL: It was further away, ja.
- VM: When you started working in ORL, did you see Calvin often?
- KL: No, definitely not often.
- VM: Make a guess! Every week? Every month? Every day?
- KL: Every fortnight, rather.
- VM: He would come around and talk about the scientific detail?
- KL: Ja.
- VM: Of course you saw Al all the time; you were working with him.
- KL: Yes.
- VM: Do you remember the seminars?
- KL: Yes.
- VM: You gave a seminar?
- KL: I gave a seminar, ja..
- VM: Did you enjoy the seminar?
- KL: I was extremely nervous. I think I gave a seminar on my famous spot. There was a party the night before and I *had* to go to the party; everybody said you have to come there. "I have tomorrow a seminar"; "come to the party". So I went to the party. I got up at 3:30 or 4:00 o'clock in the morning to put together the details for the seminar. It went all right finally.
- VM: Just one seminar you gave there?
- KL: Yes.
- VM: You mentioned before we started recording that you had to leave Berkeley hurriedly because of the Dutch army or something?
- KL: Not hurriedly, not hurriedly. I had to leave because during my whole studies I constantly applied for an extension of my leave but they became a little bit touchy...

VM: This is from the army?

KL: From the army, ja. At the time I should take a position, I skipped the army. But then it would have been impossible to return to the Netherlands for a number of years. I didn't want to do that. I had met in Berkeley a Dutchman, it was a New Year's party, and somebody said, "ah! A friend of mine who lived in the same house, A German Klemm...

VM: Karl Klemm?

KL: Karl Klemm, and so we did a lot of things together. A lady was responsible for foreigners there in the lab. said there was a party and there will be another German so Karl Klemm said "hah — another German". We came there and that other German happened to be a Dutchman that I knew very well because he came from the same lab. where I had been and he was working up The Hill. He had been in New York where there was a laboratory monitoring fallout from the Atomic Energy Commission and he had followed a course there in measuring all kind of isotopes, making separations, etc. Since I thought I was on this side of the ocean I thought it would be nice, so I wrote to the ministry and said how I happened to be here and there is a course from September to December about...can you fund it for me? They were happy to fund it.

So I left Berkeley and kept a month for sightseeing the United States.

SM: When did you leave Berkeley?

KL: I don't know the exact date but...

SM: The month.

KL: Must be in August.

SM: So you were there a whole year?

KL: I was there a whole year.

VM: August of '59?

KL: '59, ja. I bought a car in Berkeley from a neighbour, a Buick, a very old Buick and he asked \$100. I found out that the clutch was slipping and he said, "well \$75". So I bought I for \$75 with that clutch. With that car I drove to Canada — my idea was to see Lake Louise and go to the Rocky Mountains, down to San Antonio and to Miami and go back to New York. But in Denver where I had an aunt or a far cousin of my mother — it was a Mormon and I was fascinated because I had never seen a Mormon. I stayed a few time with this aunt. And this trip that I made out of Berkeley, I wanted to make with Karl Klemm. The idea was Karl Klemm and...what was her name? A Japanese lady that was there: Teruko (*Sato?*).

VM: I don't know her; she must have been after I left.

KL: No, she was there when I was there.

VM: Yes, but I had left when you were there.

- KL: So finally I made the trip only with her because Karl Klemm for some reason could not come along. The car broke down, blew up more or less, in Denver. Fortunately I had (*indecipherable*) and I took the Greyhound because I wanted to visit a few places. And then I came to New York then to these offices where I came with my colourful California shirt and here everybody was dressed in dark suits! (*Laughter*)
- VM: You have to get the culture right. So you went on the course? I'm not clear: when you first went to Berkeley, you had been working in a lab. in the Netherlands, before you went, and the army was something which you had to do? Before some age you had to complete it, by some age which was the limit?
- KL: No. At a certain point, they give up. I was...born in '33...I joined finally the army in December '59 when I was 26. I was in this group that was drafted, far and away the oldest because most kids were 18. When I came there, normally I should have been there in September, and I came in December, I had to go to my commanding officer. He said "you are too late". I said "Yes, but I have a letter from the ministry; everything's OK". "Yea, yea, yea, I know, but still you're too late. You missed a lot, especially in theory, the theory of how soldiers behave." (*Laughter*) This was very strange. This had to be a very strange experience, this whole army business — (*indecipherable*).
- VM: How long were you in the army?
- KL: I should have been there 24 months but, since everybody came in September, it was only 21.
- VM: When you left the army, you started your scientific career again?
- KL: Yes. I started...I joined the EEC. First I made...After I left the army I was completely fed up with mankind and I still had a Deux Chevaux (this little Citroën) that I had...
- VM: You still had?
- KL: A Deux Chevaux. When I was in Berkeley, I paid also retirement fund; I thought it was a good way of saving. With that I could buy a second-hand...
- VM: When you left, they gave you the money back.
- KL: And then I bought this car and with that car I went to Israel. I wanted to go to Israel — I wanted to turn around Turkey and Syria but on my way, there was an uprising in Syria so I parked the car in Athens and took a boat and spent some time in Israel travelling around. Then I came back and a few weeks later I got my telegram that I had...
- VM: Oh, this was the EEC. You spent the rest of your working life with the EEC?
- KL: Yes.
- VM: You mentioned earlier that you went back to Berkeley for the '89 reunion. Was that the only time you went back?
- KL: Yes, the only time I went back to Berkeley.
- VM: But you had kept in touch with some of the people?

- KL:** With John Eastman, mainly. Because John Eastman...well, we did a lot of things together. He came to Europe; he went...when I was living in, I think, in Paris at a certain point — for three years I went to work in Paris then. I think John went to Gotenborg for some time and we met somewhere and made a trip to Denmark. Later, when John was married, he stayed in Heidelberg and I at the time was living in Karlsruhe, it was close by, and we were both married and had children about the same age. We saw each other rather frequently. Later, for some time, we lost contact: John got divorced and he was looking at a certain point for a place in Europe for one of his daughters, Kathy, to spend half a year in a European school or in a school in Europe. I could arrange that so she would go to the European school in Brussels and she was sitting in the same classes as my oldest daughter. And that was very nice and we have seen each other occasionally.
- VM:** You saw Martha, you said, when she came to Europe?
- KL:** Martha I have seen very often. Martha spent...so she worked time in Europe in several places. When she was in Europe and we were in the neighbourhood she passed by — we were living in Karlsruhe and I visited her when she was, I think, in Basel or in Bern...Bern, I think...
- VM:** Bern, yes; with Karl Erismann, I think she was.
- KL:** Later, when I was in Brussels, she came with her daughter, Mitzi I think, to Brussels once and I think a few years, about two years before she died, she was again in Brussels and we just had bought a house there and we were redecorating it and she helped us with wallpaper.
- SM:** She was a wonderful woman.
- KL:** Absolutely, really very warm.
- VM:** When you went back in '89 you will, of course, have seen the round building. How did you like the round building?
- KL:** I think...I've seen a lot of changes in the whole neighbourhood there. The round building is a sign of progress and the building itself I liked and the architecture of the thing is very good. On the other hand, I think that the real original work was done in the older building. I don't know what they are doing now in the round building, but the...
- VM:** Well, the problem, of course, was that the old building was demolished to make way for the chemistry (*indecipherable*) and they had no choice but to move. When the opportunity arose to have another building, the question arose as to what sort of building you have? Everybody remembered with fondness the open structure of the wooden building and they tried to...we tried to reconstruct something along those lines. Since you lived for a time in ORL, I wondered how successful the round building appeared to you as a replacement for ORL.
- KL:** The round building...the openness was much better than the ORL.
- VM:** The group, of course, was much bigger at that time. There were probably ninety people in the round building which is much...

Before we close, just a couple more questions. Presumably you knew Calvin's family while you were there?

KL: Yes, I was invited once by the Calvin family, they were living in a very nice house. I remember that Calvin was fascinated by maps. He had a huge trunk full of maps of parts of the world he had been in and as a result, I also have a trunk with a lot of maps. (*Laughter*)

VM: So you were influenced!

KL: Yeah. We had a very nice evening. I did not have much contact socially with Calvin. I think also of my scientific upbringing in Europe. A professor is a semi-god and you meet them only (*indecipherable*). That has changed but that was a little bit my approach.

VM: Was this also true for the contrast between California when you were there and Netherlands at that time; were the lifestyles very different?

KL: The lifestyles were very different. California was much freer. So the Europeans in general are more reserved, still, although the Dutch now become a less and less reserved. What shocked me: first when I came there, they said "what's your name?" I said "Louwrier". "Yes, we know that, but when they call you?" "They call me Louwrier" It took me some time before I realised they wanted to have my first name. That was completely different. When I came to Amsterdam, I came from a little town in the Netherlands, I came to Amsterdam, I said "Sir" to everybody I ever met, even my age — I was sixteen at the time but all those people in the (*indecipherable*). I told you I came dressed in a Dutch suit...but I enjoyed that. I really enjoyed the openness. Once in a while I was shocked. "How much do you earn?" In Holland today, still, if you work for Philips, you have to promise that you don't tell anybody how much you earn. When you earn a lot you had to show it otherwise...and if you don't, it's not worth earning that money! In Europe, it is hidden.

VM: When you went back to The Netherlands, was it another shock for you? Because you also went to the army.

KL: Yes and the army was not very open and it was a shock for me and a shock for my parents. I came back to in Den Helder they were living, very small houses, and if there was a good movie in Amsterdam, I said to my parent, "well, I go to the movies in Amsterdam". "Go to Amsterdam?"

VM: How far is it?

KL: Eighty kilometers, that's about fifty miles. Fifty miles is not much in America. My whole concept of distance had changed.

VM: What you have said has been very interesting indeed and I am very grateful to you and I'm conscious that you need to go quickly so perhaps we'll stop at that point.

KL: I enjoyed it very much.

VM: Thank you.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Karel Louwrier

Date of birth 19-11-1933 Birthplace Velsen (Netherlands)

Father's full name Willem Fredrik Louwrier

Occupation teacher Birthplace Leiden

Mother's full name Hendriene Nieboer

Occupation Teacher Birthplace Den Helder

Your spouse Corneilie Oortbuijs

Occupation — Birthplace Scheveningen (Neth)

Your children Ariek, Karina, Brita, David & Frank

Where did you grow up? Netherlands (Den Helder)

Present community Brussels

Education radiochemistry doctorate

Occupation(s) EUROPEAN COMMISSION

Areas of expertise radiochemistry, geothermal energy

Other interests or activities biology, geology

Organizations in which you are active

Chapter 44

SIDNEY ALAN BARKER

Birmingham

April 16th, 1997

VM = Vivian Moses; AB = Alan Barker; SM = Sheila Moses

VM: This is a conversation with Alan Barker in Birmingham on the 16th of April, 1997.

What sort of background did you have and how did it take you to Calvin?

AB: Well, I worked with at a time just prior to Calvin, I was working in the University of Birmingham in the Chemistry Department under Professor Morris Stacy. Previous to that I had worked under Dr. Peat, later Professor Peat, at Bangor University and the late...well, they've all now died...but also my team leader under Peat was Edward Bourne who was later Professor Bourne at the Royal Holloway College, London. Effectively, the scenario was that I had been a student at the University of Birmingham all the time since 1944, got my BSc in '47, PhD in '50. I never wanted to lecture. I only wanted ever to do research. So on getting my PhD I was offered a lectureship but I refused it. Has anybody ever done that before?

VM: It's rare!

AB: But in those days it was very difficult to stay on as a research fellow. I did get a research fellow with the Brewing Research Association under Sir Ian Harborough and later I became associated with The Royal Society. Harborough had entered me for a competition; I will look up the records and tell you what the name was. I went down to London and was interviewed by august people like Sir Lawrence Bragg and those sort of people; Sir Ian Harborough had deliberately absented himself. By the time I got back to Birmingham, I was told that I had got this fellowship. I got married in 1952 after getting my degree in '50 and suddenly, of course, I was aware that you should support your wife and have a more stable background. I then sought to get a lectureship; none was available until 1954. Then Professor Stacy invited me to dinner in the centre of Birmingham with a man called Dr. Gerald Pomerat of The Rockefeller Research Foundation. I was duly told that I could obtain a research fellowship at a destination then unknown. Eventually this turned out to be in September 1955; Dr. Calvin at the University of California at Berkeley for six months until March 1956 and then went east to work with Dr. Michael Heidelberger in New Jersey at the Waxman Institute, the microbiological institute that Waxman had built with the proceeds of the royalties of streptomycin. He was, at that time, in dispute with his (*indecipherable*) about the fact that they had not benefited from any royalties from streptomycin.

- VM: What had been your particular area of chemistry, had you known anything about Calvin before then?
- AB: Not really, not Calvin. I had from the very beginning, that means from 1947 to '50, worked on enzymes of the potato initially, under Dr. Bourne the team leader and Dr. Peat, later Professor Peat. I learned the lessons the hard way of proving what you found. In fact, within ten weeks I had achieved the first proved enzymic synthesis of an amylopectin from amylose. This was to be the last peg that Dr. Peat needed to get his FRS! Everybody was waiting for this achievement. The problem had been that the enzyme, which they called *Q-enzyme*, the branching enzyme had been unstable for several years. I had started in August, because you started then in those days, you worked through the holidays because it was still just after the war, and I'd started on isolating an enzyme called *amylotransferase* from yeast. I was disgusted because I had no success in following the Japanese directions. At which Dr. Peat suggested I use the same method of isolation on getting an enzyme or enzymes out of potato. He didn't brief me that the enzyme that he was working on was so unstable. I used the worst possible thing which was to precipitate the enzyme as a lead complex, no less, and then you removed all the lead as a carbonate and you isolated the protein which you then duly fractionated, and I fractionated into the phosphorylase enzyme, the one that synthesised the linear component of starch, and then the one that they called Q, or the branching enzyme, the one that converted amylose to amylopectin. They were remarkably stable having gone through this lead complex. After some preliminary work, I was told to duly synthesise amylopectin in large enough amounts to do an extensive examination and proof of the structure of amylopectin.
- I therefore came in one day at 7:30 a.m. in the morning. I and a technician peeled one hundredweight of potatoes between us, remarkably by midday we had isolated a very large amount of Q enzyme; in the afternoon of that day we incubated it with amylose, monitored it and examined and isolated amylopectin by that night. Within a few days I had characterised amylopectin and my team leader, Dr. Bourne and Dr. Peat were overjoyed and rushed off a paper to *Nature*. I thought how wonderful it was to be a research scientist! Unfortunately, the next Easter following — that would be Easter 1948 — shortly after the paper had been published in *Nature*...
- VM: With your name on it, of course.
- AB: ...with my name on it...there was a complete rebuttal from Kurt Meyer and his main assistant. They said that the enzyme I had isolated really needed phosphate to do the conversion and I had said that it didn't need phosphate. Phosphate, of course, is required to break down amylose to glucose-1-phosphate. So everybody then was wondering whether the little boy wonder would come up and really prove it and grind the rebuttal into the ground. I had a very good teacher in Edward Bourne, and he and I ruthlessly, relentlessly, month after month did work that eventually absolutely proved that the enzyme did not require phosphate. We published papers to that effect in the *Journal of the Chemical Society* to that effect. Eventually, one by one, because there had been a follow-up by another person saying that...rebutting the work, eventually Kurt Meyer made a complete and utter rebuttal at the American Chemical Society. I think, when Stacy was there. We had presented all the evidence in 1949 at the International Biochemical Congress, I think, held at Cambridge at which Kurt Meyer was there when he listened to the evidence. He then went back, got somebody else to check out his assistant's work and found it was faulty. Therefore, the following year in 1950, when Stacy and he (Meyer) went to an American Chemical Society, Kurt Meyer said we were right and he was wrong.

- VM: To move you on toward the time you went to Berkeley, you were, in effect, a carbohydrate-enzymological chemist-biochemist.
- AB: That's right. After my PhD for the Brewing Research Association, I started working on bacteria, isolating enzymes, particularly from *Aspergillus niger*, doing various enzymic conversions, mainly breaking bonds and making a new one, conserving the energy — generally without any phosphate being present.
- VM: Before you got to Calvin, had you had any prior communication with him about what you might do there?
- AB: No. I arrived on the doorstep...
- VM: Married?
- AB: Married with my wife. We had landed at New York to be greeted by Professor Michael Heidelberger, we stayed the night at Central Park West there, we saw the Rockefeller Foundation man in New York who duly provided us with the necessary tickets to get to Chicago on the train. I remember we bought some provisions including some frozen orange juice which duly exploded in the toilet that was attached to the compartment. At Chicago we went, I think, to Buffalo to see Niagara Falls; I'm not quite sure but eventually we arrived in San Francisco, got to Berkeley.
- VM: Anybody meet you?
- AB: I don't recollect now. Calvin had a wonderful secretary who managed to do sort of everything for all these foreign students. At the time I joined Calvin, in September '55, he'd got about 30-32 students. There was still a semblance of wartime. Bassham came in his uniform, his reserve uniform occasionally. (Now I have to refer to photographs to remind me; just a minute.) Alice Holtham sort of literally mothered we foreign students, looked after wives, got me a flat, my wife and I a flat, just opposite in Hearst Avenue, I believe, overlooking...one could always smell the eucalyptus trees which I do now when I go over to America. And really took care of the group. Some of the people there at the time, and we were in the old hut, the Old Radiation Laboratory still, I remember I spent my 30th birthday there and suddenly realised how old I was. They had a party in the lab. for me, I remember, it was April 13th.
- VM: Who were the people in the lab. with you, do you remember?
- AB: Yes. Right opposite me was Pekka Linko and on the other side was an American student whose name I forget at the moment. One of the things I learned from going to America was to look at men as well as their achievements. I always had this thing, which I always tried to practice, that I wasn't teaching people chemistry I was teaching people chemistry plus how to use it, how to live, how to treat people, how to train people. I therefore was very interested in this man who was destined to be a Nobel Prize winner. Without Calvin knowing it, I was at that time sizing him up for Professor Murray Stacy who was on the Council of The Royal Society. And he (*Calvin*) was at that time, very soon afterwards, to become a Foreign Member, I think, of The Royal Society and eventually to get his Nobel Prize.

One has to put in context that it is very difficult to control 30 bright people of all nationalities. One gets as much from working for the great man as from the people you are actually surrounded by, from all these different countries. Calvin did have a

nasty habit in my estimation. He would come through the back door and he would go immediately to the person who was having success in his research. At that time it was Pekka Linko and he would say "good morning, Barker" and not talk to me. He would say good morning to his American student and off he would go. So it became my great and burning ambition that this man would come to my desk and he would come every day and I would lead him and show him how to apply what he was trying to achieve to the benefit of mankind! That was my burning ambition, although he didn't know that at the time.

VM: You haven't told us what you were working on when you went to the lab.

AB: Calvin knew what his photosynthetic cycle was except that he couldn't prove the last piece of the jigsaw. If one goes from A-H in compounds one knows there's a missing piece to complete the crossword or jigsaw. He knew what it was, it was erythrose-4-phosphate, but he couldn't prove this. One of the things that Calvin benefited from was that each foreign student coming from a different country brought a new technique. He might at that time have the latest NMR because he was just getting from Perkin-Elmer the NMR which wasn't going to help him very much at that time. It didn't help him, certainly on the photosynthetic cycle. But I brought paper electrophoresis. I had become quite clever at separating things and devising ways by paper electrophoresis and so on to separate them. This is just what Calvin needed; he needed the ability to separate a radioactive spot that he presumed would be the area where he would find the last missing piece but he wanted to prove it without isolating it. Therefore, you had to do several things to it to follow the radioactivity, such as make it migrate as a negatively-charged entity, find out the strength of the acid by re-running it in an acid solution rather than neutral, show that it still ran as a charged molecule so that it was not something like a carboxylic acid — it was a stronger acid — treat the unknown radioactive spot with a phosphatase of wide applicability, get a neutral entity which then you separated and, because that was unstable and that was what was beating Calvin. He didn't appreciate that some of the small carbohydrates were highly unstable and what you needed to do, as they were produced by the enzyme, you literally reduced them to stable erythritol. Very soon he had a bag of proof. But the key things that I learned was, of course, it was Bassham showing me the practical way to do things, Bassham taking me into the more highly radioactive laboratory where the phosphate work was done, watching me that I didn't contaminate myself and other things, because I was a complete novice. He was a very good and practical teacher even to somebody like myself who was supposed to be a postdoctoral fellow. Calvin was literally a human dynamo, I suppose. It was a little disappointing initially that he had given me this project. I know he needed to solve it. It was the thing that had to be done. But effectively, other than introducing him to the techniques of paper electrophoresis and teaching him the instability of small carbohydrates, he and Bassham were telling me what to do almost like a technician.

VM: You published this work on electrophoresis?

AB: This work was published in *Biochim. Biophys. Acta* (21, 376, 1956); it carried the names J.A. Bassham, S.A. Barker, M. Calvin and U.C. Quarcck. Calvin, 40 years later, was asked about this work because this was the last piece of the jigsaw he needed. He had the conception, the brilliant conception of how the sun's energy was trapped, but he needed just this last bit to prove his complete sequence. He was asked 40 years later, that's in 1996, I believe, what was the most difficult thing. The last piece to him was the difficult thing because he hadn't got the techniques and he hadn't got the carbohydrate knowledge. This man Gerald Pomerat was a very, very clever man for the Rockefeller Foundation and he literally knew who to put with what. He not only

knew the great scientists in America but he was travelling constantly in Europe and he was a catalyst. It is significant to say that after that The Rockefeller Foundation never did much good for America before it was doing good in this way of fertilising European and US scientists. In 1956, following the Hungarian rising, all the Rockefeller Fellowships for Europeans were stopped and the money used for the people who had escaped from Hungary. I don't know what happened subsequently, whether it was resurrected. I felt it was a great loss to America, and a great loss to England, because it was cross fertilisation of the best and highest quality.

So he had taught me the technique of two-way paper chromatography which was the essence of...

VM: This was Bassham?

AB: Calvin and Bassham had taught me this technique of two-way chromatography which enabled them to separate all the radioactive compounds. I thought that was something. It was like a fingerprint of a cell at any time. I think that Calvin had got this brilliant conception of all he wanted in life was to show how the sun's energy was converted into chemical compounds. He couldn't see that technique that he had developed there, plus the techniques that the students had brought to him, would enable man to benefit in a practical way from this work.

Therefore, I set about secretly starting my own research. What I did was to send to Dr. Timpson at the Mellon Institute and ask for the latest anti-cancer drug. He sent me in 1956 azaserine and I couldn't have had a better compound to put into the photosynthetic network. Calvin had two strains of algae that the two coloured girls (*Alice Smith and Altha Vann*) cultured every day and gave us students. One was called *Scenedesmus* and the other was called *Chlorella*. Luckily the first one I used was *Scenedesmus* and I did attract the pathway of radioactive carbon in the presence and absence of azaserine. Then this brilliant method of separating in two directions gave me a map which showed that the azaserine was blocking at all points the transamination step. From the keto acids out you went to any other metabolic cycle, you found the same thing. That was the thing that brought Calvin to my desk. Then he would come every day and he was very, very excitable, he used to get very excited, there used to be long discussions. We published that in the *Journal of the American Chemical Society* in 1956 (78, 4632).

VM: And who were the authors on that?

AB: The authors on that were as before: S.A. Barker, J.A. Bassham, M. Calvin and U.C. Quarck. By the way, I forgot to mention in this article that I mentioned where Calvin is going back 40 years, there are pictures of myself and Carol and Bassham and the four of us, yes.

VM: You several times talked about Calvin and his brilliant concept: how did you find him when it came to nuts and bolts of doing scientific research? I appreciate you've been talking so far about the big picture. What about the little pictures? Was he good at that?

AB: That's a difficult one. Because, you see, by the time I knew him, my concept of the man was that he had worked under very difficult circumstances and achieved brilliant results. He had managed in an old dilapidated wooden hut to achieve a dream and he was rather like men I met afterwards (I'll tell you about that in a moment). So by the

time I met him he wouldn't be a man who would be showing me or drawing me little pictures.

VM: When he sat down at your desk and looked at your own data, was he good at analysing it, was he good at suggesting with you what might come next, things of that sort?

AB: Not really because, you see, I wanted him to come to my desk; therefore I had done all the planning myself. I had sent away for the antibiotic, I had done all the experiments and told him about it afterwards. Of course, the idea that I had, and what made Calvin very excited, was that he'd got a technique that would enable drugs to be assessed in their metabolic action. I don't think he subsequently did anything with this golden opportunity. I myself couldn't do it because I would have needed three or four years to have set up all the techniques that Calvin had in his possession. At the time he was extremely excited about that possibility because it would save a lot of animal testing. And we are still talking about that in 1996. It's a thing that stuck with me all the years, that there seemed to be a complete inaction on the part of biochemists in trying to find practical ways to find out how drugs worked.

VM: Not entirely: when I was...

AB: Not entirely. As an example when I later was working on the biological activity of compounds and I was working on (*indecipherable*) acid and went to the virology unit at the East Birmingham Hospital, then I was told of things like eggs that you injected an unknown compound to find out about its toxicity, one of the simplest ways of finding out whether something is toxic or not is actually to put it in an egg, not to use on an animal. I did over the years see several promising techniques along those lines. But nobody seems to put it together as an act, a true act — what I am going to do is to develop and test drugs and I'm not going to use animals, that logic...until the last extremity. It's always animals first. People seems to be trained to test the things on animals and they don't seem to take advantage of these techniques that are lost in the literature.

VM: Were you aware of any other group, outside the Calvin group, developing or using that sort of technology that you used and that many others used so successfully?

AB: No, no, not at that time.

VM: Later?

AB: No, no. What happened was, you see, I didn't have the normal year with one person. I had been told, almost, I should go and work with Professor Michael Heidelberger (*Editor: Michael was Charles Heidelberger's father — Heidelberger was one of the four original PhD chemists who started the Bio-Organic Chemistry Group in 1946-1947*) because that was the next six months. Stacy had been taught by Heidelberger so, as it were, his "son" in certain terms was also to go. And, of course, Heidelberger died recently aged 100. When I went to the Microbiological Research Institute (*at Rutgers in New Jersey*), it had been newly built, I saw another Nobel Prize winner, Waxman, and, again, one saw a completely different man, a man again with a dream, nothing else but the dream mattered. So people didn't matter. But until you had seen his autobiography, you didn't understand this man. He was like an insect...

VM: This is Waxman?

AB: Waxman...he was a Jew, he had learned everything; he had been prevented from going to school as a Jew in Russia so he'd learned everything. You saw him being what *you* thought was rude, leaving a lecture because he couldn't stand receiving knowledge through his ears, he was used to receiving knowledge through his eyes. I saw in California the man *I* think was the top man that I ever met that was worthy of being a Nobel Prize winner, and that is Pauling. Pauling came to Berkeley. It was wonderful. He gave twelve lectures on how he solved problems. It was enthralling because here was a man revealing how his mind actually worked. Calvin never did that. Calvin had goals and concepts and how to achieve them but Pauling was revealing to ordinary scientists his innermost secret.

VM: How was Calvin with people in your experience? You said that Waxman was...

AB: Calvin was very caring and very kind but he was like a modern business man, no time if there was no results. He'd got to the stage where, as I said, he would spend as much time as possible with the people who were getting, as it were, the success for him and not...he was not a teacher — at that time. That doesn't mean to say that early on he wasn't a teacher but he wasn't a teacher in that respect.

VM: You must have presumed that there were things, nevertheless, that you learned from being in the place.

AB: Oh yes. The other problem, I think, was that America was about to break out into a turmoil and the two girls, coloured girls, were in the same laboratory room as myself and Pekka Linko and the American student. Every day they used to grow the photosynthetic algae and it was a sacred trust. One, as I said, was *Scenedesmus*, on which I had done the work on azaserine. The work that wasn't published was on *Chlorella*. You see, the azaserine, to do its action on *Scenedesmus*, had to get through the cell wall. When we used *Chlorella*, it didn't do a darn thing. The controls and the results were exactly the same. And again, it was as though I was trained to teach Calvin a lesson. I know that's pompous but it is a lesson. There's a lot of lessons in nature that I almost store up for myself from other people. They become almost, as it were, generalities. It's a thing that I learned more from Heidelberger. I mean, Heidelberger at the same age or an older age and Calvin but, with only two or three students, was a great teacher. He taught you with his own fair hands, very much like Bassham did, and he was teaching you immunology. But he was teaching you in great detail in explaining the immunology and the immunochemistry and I was explaining to him the structure of the *Pneumococcus* polysaccharides which we were using.

VM: In the case of Calvin, as you say, there was a much bigger operation and Calvin was like some sort of...

AB: That's right. It was like a business manager, I'm afraid.

VM: You've told us something of what you learned directly from Al Bassham. What did you learn, do you think, from the experience, the experience of being in that lab. at that time?

AB: It was wonderful. Within that area, round Berkeley, there were seven Nobel Prize winners at that time, most of them were up on The Hill in the proper Radiation Laboratory where money was no object. There were great men on the campus and we used to, my wife and I, although she wasn't a chemist, we used to go and listen to

these people. People like Pauling telling you how they solved problems; I mean he would take the example, which I could clearly understand, that you took an X-ray photograph of an unknown compound and ordinary people, like the people I had been associated with the X-ray department at the University of Birmingham, took three years to do the structure of glucose-1-phosphate in those days; you know, we are talking about Pauling had this concept that he would be like God, he would arrange the atoms in the molecules in the lowest state of energy or in the way that he thought they should be arranged. He would then predict what the X-ray picture should be and he would give himself about ten tries. Now, if he failed, he failed. But it didn't take him long to do that operation and, if he found one that fitted, he would have done it in a month whereas somebody would have taken three months. Not only that, he was doing much more than a man who was sort of pedestrianally solving a X-ray picture. He was pushing science forward at a much more rapid rate. He was showing he understood why the atoms were arranged in this way, not just finding out that the atoms happened to be arranged in this particular way.

VM: It sounds as if the lectures you went to by Pauling had as much, perhaps even more, influence on you than the time you spent in Calvin's lab.

AB: I never thought that anybody could manage 32 people and I don't think you can. You cannot. You have to be selective. You've got an objective. It's like somebody running a company, you're very efficient, you've only got a certain of time. I don't know whether he had had his first heart attack at that time...

VM: I think he had, yes.

AB: ...and therefore, he would be harbouring his strength. One has to take all these things into account when you are assessing the man. He gets his Nobel Prize, as far as I'm concerned, with a brilliant concept and getting the means to achieve it relentlessly. That's what he gets it for, not as a great teacher.

VM: As a great thinker?

AB: I couldn't assess that process because I am coming in at the last bit so I'm not allowed to do that. You don't know who contributed what. Certainly, there was lots of talk about the man that left (*Benson*) that he had a great deal to do with the grand work. He'd left by the time I arrived.

VM: This was Andy Benson?

AB: That's it. Again, I can't say that. Bassham wasn't a thinker. He was a teacher and I believed that Benson and Calvin were largely responsible for the result. The thing was carried on by its own momentum with this influx of foreign students.

I spoke before about Calvin's secretary. With a man like that, running 32 people, you do need to have a very efficient and caring person, somebody who is not only just a secretary to type endless papers you are going to produce with all these number of people. If I produce two in six months...I don't know what the output was in a year from Calvin's lab. with 32 people. You needed somebody extraordinary and she was extraordinary, Alice Holtham. I recollect on Thanksgiving Day on the November we students were taken to her mum and dad's house, Mr. and Mrs. Holtham. Our great friends there were Carol Quarck, who is on two of the papers, Nel, I think it's van der Meulen of Holland.

VM: Yes; we're going to see her in a couple of weeks.

AB: Remarried? Remember me to her. Her name is...

VM: Prins-van der Meulen.

AB: I used to be a consultant at Gist Brocades and I used to travel every six weeks to Delft in Holland but never deviated, I'm afraid. This (*photo*) is Thanksgiving Day with Mr. and Mrs. Holtham — you can see Carole there; now you can see also the two coloured girls, Altha (*Vann*) and Alice (*Smith*), is it? I'll get their names correctly in a moment. But typical of the Holthams is that colour and creed didn't matter.

VM: Do you remember this character here?

AB: Yes; I think he's coming up in a moment.

VM: Do you remember his name? I'll tell you; it's Ning Pon.

AB: That's right, it's all written down. This is the apartment in Berkeley, one-bedroomed apartment. We overlooked the campus and therefore we didn't have a car which was again different from all the other students. That benefited us a great deal because, among the things I learned, I learned about American people and I wouldn't have learned about American people if I had a car; I went past them. By not having a car we had to do something different from all the other students. There was a wonderful lady at International House in Berkeley for the foreign students. Every other weekend we went in a troop of 30 or 40 in a bus to places like Paradise in Northern California. The Round Table there would then meet us. This is the really extraordinary thing because we were black, yellow, from Afghan; we were all colours and creeds. These ordinary citizens of Paradise or the other towns we went to would on no account have had coloured people — some of them wouldn't have had coloured people as guests. But, as we got off the bus, it was number order and you were numbered with the host and that was it.

Tape turned over

Typical of the things I learned was how you controlled peoples' minds. I had come to do chemistry but during the last world war, as a boy of thirteen or so, I was very interested in the Philips short-wave radio we had. I remember my father and I used to listen to America, Australia (the cricket there), to Haw-Haw, we listened to Moscow; there was no barriers there. When I got to America, of course, the era had come and gone. There were no short-wave radios in sight; everybody's radio was now medium or long-wave. Here, you have a great country and somebody in Wisconsin, at the time of the election, somebody in Wisconsin cannot find out what somebody in California is thinking because you've either got a national network or you've got a local network of California news. What you *haven't* got is an ability for news to transmit between California and Wisconsin. Perhaps the Internet will do that for us.

VM: Incidentally, before you leave it, one of the things that many people have talked about is what you termed the "old hut", the Old Radiation Laboratory. People have felt, some people — I wonder how you feel — that it was a factor in determining the quality and the character of the group of people who worked in it. Do you think that?

AB: I think: yes. One of the things I think you learn is that machines rule man and the other thing you learn is to keep men well away from machines! If you automate a

process, the one thing you shouldn't do is to put a man near the machine because he still believes he's working a full day's work. I practice that. When the machines came to automate analysis, I found, if I put them in the lab., I got no work done. By putting them into a room somebody had to find something to do. I know that it sounds again horrible but I found this and if I answer that question what I'm indicating is that we were still in an era where we *made* the machines, where we *made* the electrophoresis machines. We didn't buy them; we made them. When I got over there, all I did...there was no company to order them from, Calvin had got a workshop, I went to the workshop people, told them what I wanted, set up the apparatus — and that is completely different. You're in an era where you've not reached the black box, where you've got an automated system, where the people who do analysis don't know what's in the box and their mind suddenly stops working because it's like the software today — it is an impediment to original thinking. If I had my way I wouldn't teach students with machines; I would teach them in another way, the way, in effect, in the era we were taught. We did things ourselves, everything from paper chromatography: my father had to make the tank because it was a bigger tank, I wanted a different thing, I was doing two-way chromatography just like Calvin but not for that purpose. But it was in an era if you walked into the laboratory, you had the flask but you didn't have the machines. You had spectrophotometers, very little else.

VM: So you had to be much more hands-on and much more creative, you think, than people are now.

AB: Yes. But Calvin did have the workshop facilities to do that...

VM: ...and without that it would really not have been possible.

AB: ... it wouldn't have been possible. I would have been lost; I wouldn't have carted my paper electrophoresis machine over and by the time it had come, as it were, it would have been too late.

The flat we had was very interesting. I remember the bed came down off the door; as we moved in the bed, it creaked. Eventually we got into the habit of taking the mattress and putting it on the floor and, thereafter, for six months my wife and I slept on the floor. We were very happy. On the Rockefeller Fellowship, which was non-taxable at that time, we tried to save for our first car. Because we hadn't bought a car in America, we had saved enough money between September and, I think, February of the following to buy a new Austin A40. I duly deposited all these dollars down in San Francisco for delivery of a car in Birmingham, England when we came back in June. Then disaster struck because it was the year of Suez. There was an emergency budget in February or March of 1956 and then I really learned how to live on no money. Because we had to...and VAT went on — or the equivalent of...purchase tax in those days and my wife and I really learned how to live on practically nothing. We really studied the Americans, worked out what they didn't like; it happened out to be what we liked. They don't like offal, they don't like lamb, they don't like...that's a generalisation but we're talking about kidneys, liver and that sort of thing. The Americans don't really go for them or they didn't go for it in that time. That was the sort of thing we enjoyed. Instead of California wine, I think we bought a gallon of cheap sherry or something. We certainly enjoyed ourselves and we just achieved, before I went east, the remaining money.

On the social scene, we all held parties at each other's house. There was never any ill feeling among the students at any time, the research students, research fellows. To me that's part of the success. Everybody was willing to help everybody else. It was, you know...sparked off great camaraderie. The people — I'm looking at the picture outside the Radiation Hut, ORL, in 1956 — there was Altha (she was one of the coloured girls who grew the algae), Al Bassham, Ning (*Pon*), Pekka Linko, Carol Quarck, Hans Grisebach (who she married), Alan (that's me), Jean (*Bourdon*) (the Frenchman), Ozzie (*Holm-Hansen*), Alice (*Smith*) (the other coloured girl) and Paul (*Hayes*) and Kazuo (*Shibata*). That was the group at that time in early 1956...

VM: In that lab.

AB: ...in that particular lab.; there was another lab. elsewhere. There was another man, I'm looking now at another picture outside the hut. there's Ning and Ernst in addition to the names...yes, that's right: Ernst and Ning were there. And I've got pictures of all of these people; I think this was the greenhouse where they were growing the leaves. One of the things about these two coloured girls is at lunchtime I noticed a strange thing. They almost pleaded with Pekka and myself and Carol and so on to use this algae. If at lunch time we lads and girls hadn't used it, I noticed a strange ceremony. And it was that they made a soup of the *Scenedesmus* but they never made a soup of *Chlorella*. If you recollect, it was the *Scenedesmus* that responded to the azaserine, not the *Chlorella*. Much later, when the space ship went up, so the space men went up, I always thought that if I had to complete the photosynthetic cycle inside a space ship that the one I would first go for was *Scenedesmus*.

VM: When you say they made a soup, you mean to eat?

AB: Yes, they drank it, they drank it. They cooked it and ate *Scenedesmus*.

VM: Did you taste it, ever?

AB: No, I don't think I did. But when I asked them, it was always the *Scenedesmus*, never the *Chlorella*. I remember talking with the Russians in 1960 about this when I went over to Moscow and Kiev. It's quite fascinating.

Alice Holtham, the secretary of Calvin, was a real driving force and she saw that, for example, my wife was introduced to one of her friends who just had a baby — you know, the usual showers and all this sort of thing. We made friends outside the circle of people. I'm looking at another picture with another name that I haven't mentioned yet, Masao. But you can see here that we are visiting around — Masao, myself, my wife, Carol and Nel going to the Carmel mission. We went to...but we met some...I learned a great deal, as I said, about Americans by going on these trips with International House. One of the first ones we went was man who had made a fortune out of insurance, had retired and then, like so many Californians, had started to think why we were here, the origin of life. He'd started to study all the religions in the world. In fact, he'd got a wonderful library and at that time he was into spiritualism and, typical American, he had gone down to San Francisco for a seance and, of course, nobody ever does this but Americans do, he had taken his tape recorder. And I remember he played the tape recorder on how Atlantis came into being and the medium told a wonderful story — didn't matter whether it was true because it's just what...again full about things that happened in this world. People feed on other people, in a sense — whether it's harvesting people's minds. One sees how power is manipulated, how people are controlled.

And going back to this, in one of my visits to the north we were taken to the local newspaper office, say Paradise as an example. In those days you would see the news coming in on the teletype. When you asked what the news is, the news had been sold to them, as news is, and I tracked it back because I then went to San Francisco and saw the big newspaper. You see that news came into America in those days at two points only: San Francisco and New York. It was then sold and so the news for Paradise was somebody who owned the news in San Francisco selling it to somebody up in Paradise. And, you know, it's the same thing today. That gentleman in Australia who buys up all the news media is, unfortunately, the Internet has sort of overtaken it but I think it's the thing that will break down all barriers.

VM: We're getting a long way from...

AB: I know it is but you asked me what I learned and I learned a lot more than chemistry and that is what I always transfer to my students. At this time, for example, the Russian Ambassador for Saudi Arabia is a student of mine; I taught him more than chemistry. I think people of the calibre of Pauling and people of the calibre of Heidelberger (who, I thought, should have been a Nobel Prize winner) teach more the whole man. It's very difficult to find people. I learned a great deal from these people.

I also learned: if we take Pekka Linko, he was a wonderful fellow; somehow, you seem to be able to remain friends without seeing people for years and years and years. Yet, like I teach my students if they are ever called on to help one another, whatever religion, creed, Communist, Jap, Iraqi, they help one another. Because woe betide them if they refused to do this. Now, Pekka Linko was this sort of type. When we were on the east coast and we lived on a veterans estate, the veterans had come from Korea, and one day Pekka Linko drove up in a wonderfully new car.

VM: One second: was he living on the same estate as you?

AB: No. He had finished with Calvin...

VM: He was visiting?

AB: ...and was on his way to New York. This is typical of people who worked for Calvin. They were all different but there was something magic about Cal...or Hans...the other people; there was something — it wasn't chemistry it was the things that motivated, the way their minds worked. If you took Pekka: Pekka was always low on funds and that's why at parties nobody bothered about who paid for what; if you'd got money you put it there — if you didn't, then it didn't matter. Pekka was always low on funds. So when he got to Chicago on his way back he had no money, no money literally no money, all he'd got was a ticket going from New York. And so, a man like that has to use his mind. So he bought, with his last few cents, the *Chicago Tribune* and there he found that people were advertising you...wanting you to drive new cars to deliver in New York. So, he flashes his international driving license and they gave him a beautiful new car and so, on his way...That strikes me: you know, that sort of man is different; they are different.

My students were the same, you know. I had a Lebanese from the American University and he was a Christian. I had a Syrian who's now a professor in the United States: his name was Atassi and he was of the ruling clan of Syria before Egyptians joined up with the Syrians. Now he wanted nothing to do with politics and he wasn't army. And when he got his PhD and went back, they wouldn't let him go to the pharmaceutical company that had engaged him, demeaned him and made...So he had

to escape. Now, the man who helped him escape was my Christian and he sent his clothes out with an old lady; he escaped out of the mountains, he got to Beirut, it was Christmas. The man in Beirut sent me a letter...sorry, Atassi, this man, the Syrian, sent me a letter Christmas time, "Thank you, Dr. Barker (in those days) for the offer of a position in your laboratory." I hadn't given him one but, you know; and I then showed it to the university authorities and I used it to get him through immigration and got him here eventually.

The two technicians, coloured technicians, who grew the algae, there was some problem with them and Calvin. That was the time I crossed swords with Calvin; I defended them. One of them (*Altha Vann*) was, I think, about to be dismissed and I and the other students rather ganged up against Calvin. I had gone to a coloured church, my wife and I were the only white people there, because I just wanted to see what America was like. And I was very fortunate because after I left Berkeley all hell broke loose in Berkeley and it wasn't the Berkeley that I knew when I was there.

VM: When you left Berkeley, you worked first of all...you finished your Rockefeller period in Rutgers, did you?

AB: Yes.

VM: ...and then, presumably, you came back to England

AB: That's right. I was extremely fortunate. Within a few months Dr. Edward Bourne became professor at the Royal Holloway Colleges in '56 or early '57 and I took over his research team. Wonderful, made research team of 12-13 people and I kept that sort of numbers all through my working career. Eventually I had one, two or three, even three team leaders working with me.

VM: In Birmingham?

AB: In Birmingham.

VM: All your working life was in Birmingham was it?

AB: It was indeed except that, in '66-'67, I went to Canada for six months. Prior to that six months, I spent three months as a visiting professor at the University of Chicago, giving lectures, living in Oak Park. Then I moved up to Kingston, at Queens University, Ontario with Dr. Jones, Professor Jones in those days, and I went right across Canada, lecturing at Vancouver and...It was the time of Lemieux so I went to his place, Saskatoon. I went to Quebec. I gave a lecture at all these universities on behalf of a sort of Commonwealth Research Fellow; there's one chosen each year.

VM: Did you ever become a lecturer in England?

AB: Yes. I became a lecturer in the year before I went (*to the United States*) so in '54 I became a lecturer, I had a year's leave immediately for '55-'56. I became professor in '69. Prior to that, up to '60, I had been a man in an ivory tower. But I bought this house in Sellyoak in that year and I had intense kidney stones wondering how I could pay for it! So for the first time I became interested in making money. I therefore decided I would become a consultant. You don't become one! You are invited! While I was being "invited" I wrote two books, *Polysaccharides and Microorganisms* and *The Carbohydrates of Human Tissues*. I didn't make any money but it taught me a great deal; I became an "expert" in those areas.

Then, very soon, I did become a consultant. I did this by an underhand way. I was asked to write an article in *Endeavour* in those days — I didn't write one, I wrote several — and I put there theories that I wouldn't have been allowed to put in the *Journal of the Chemical Society*. This attracted the attention of Abbott Laboratories in the States, Hercules and so I eventually became a consultant for ICI, Wiggins Teap. In the year I retired, I was consultant for about fourteen companies. I stopped going to America in 1974 and concentrated on Europe and England.

I received a further impetus at the age of 62 when ten people had to be, as it were, prematurely retired from the department. Since I was the oldest, I volunteered first and I spent the next three years, from age 62 to 65, working one day a week and then I used the university facilities on my own to really become a consultant.

The other thing I was interested in was that, when I went to Canada for the purpose of lecturing on behalf of the Commonwealth research place, what I was after is teaching myself how to get information. So I spent six months, part of it, in the library following up stories like a newspaperman does, in effect, almost spying but learning the technology of information gathering. For example, you would see the results of twenty stories in that six months where I tested myself: I actually followed information up, I got stories and I sent them to England and they were all published in that period. They weren't published as research papers; they were published in newspapers. But I tracked the story of, for example, du Pont's hollow fibre. There were patents saying that du Pont has a fibre to spin hollow fibres but nobody said what they were for. I divulged that they were for reverse osmosis. I followed the story up. There was an advertisement in the bee research journal over there at that time that somebody wanted tenders for a kilo of bee venom and I cracked that story and located where and what for. So on and so forth: I did twenty stories. I enjoyed searching and looking at...I remember another story, was an Australian clinical trial going on in Sydney amongst school children. It didn't tell you what was being tried and what it was made from. I studied the patent literature and found that it was sucrose-6-phosphate that was being tried. It would stop about a third of human caries and I found also that it's in natural sugar in the sugar cane but that it's refined out.

VM: Last couple of minutes. Are you still doing chemistry?

AB: Yes. I still...my wife died of cancer and I nursed her here for fourteen months. During that time I went a step further. I started, before the Internet started, I actually do searches of world patent literature now and disseminate them to various companies. But during that I wrote a newspaper each week; I wrote a science newsletter for CPC Press, which was owned by the *Los Angeles Times*. I used to transmit every Sunday a twelve-page summary for executives of patent literature in biotechnology and used to transmit it from upstairs in 1994. I never used a computer before, had to teach myself how to use one and then went from FAX to modem and 12 pages used to disappear in seconds.

VM: And you still do that sort of thing?

AB: No. I had to stop it when my wife was in the bed and she was suffering intense pain from cancer of all sorts. That, I suppose, is where my training as a scientist makes me abhor my fellow men who go for money. Because the one story that I would like to break is which drugs are made and sold to the public and which are not. It is always, always for money. So I, who worked and gave my life for ten years working with doctors after I came back from working with Calvin, all on the human being and set

up a network, gave it freely, when I came to an anti-inflammatory drug and I'd worked out with doctors exactly how it worked, how to assess it, everybody was pleased. The minute I said I wanted to cure rheumatoid arthritis, and I thought I knew how to approach it, no money was available, neither from charities or from companies. The tragedy is, you have an anti-inflammatory drug every day. If you cure somebody, your business is gone for ever. It is the same with cancer, it is the same with pain. My wife died of cancer in terrible agony and I was assured that if she went into hospice they would cure her. They have no idea or concept of how to cure pain. All they do is to make morphine, which is the cheapest painkilling drug. I was sitting up there looking at patients which show there are ten different ways of causing pain in the human body and there are antidotes to these. There are drugs there that man will never receive because then they will stop somebody making money. This is the tragedy of a life like mine. You learn all these facts that are not privy to other people. I think, you know, it's terrible that I haven't had the courage to throw my life on the line. You know, because you work for these companies.

VM: Anyway, I think we're going to have to...Thank you very much indeed.

AB: ...I don't reveal to other people.

VM: It has been very entertaining and gratifying to meet you after such a long time. Thanks a lot.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name SIDNEY ALAN BARKER
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Areas of expertise Radiation Chemistry, Enzymology,
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Other interests or activities Patent Litigation
Organizations in which you are active Derbyshire Archaeological Society,
Family Tree research.

Chapter 45

F. ROBERT (BOB) WHATLEY

Oxford

May 9th, 1997

VM = Vivian Moses; BW = Bob Whatley; JW = Jean Whatley; SM = Sheila Moses

VM: This is talking to Bob Whatley in Oxford on the 9th of May, 1997.

You, of course, were one of the people who was not actually in Calvin's lab. but who was looking at it from the outside: when did you first see it — or when did you first begin to be there and observe it?

BW: In 1948, the end of the year. I went from Cambridge to Arnon's lab. One immediately became aware that here was a going concern because, of course, they were starting to work on CO₂ fixation and it was beginning to go. I didn't have very many firm contacts with people in the lab. but it was quite clear that in Arnon's lab. we were very much interested in the CO₂ fixation work. The biggest problem, as far as I was concerned, was, of course, as a "new boy", learning new tricks, in Arnon's lab., we were simply doing continuation of work on the Hill reaction. That didn't actually bring us into any very firm contact with Calvin but, at the same time, one was, as I say, very much aware of interesting things going on there.

Subsequently, I went away from Berkeley in 1950 for a two year period, two and a half years, to Australia. There I, in a sense, lost contact with this immediate stuff but I was happy to get back to Berkeley again in '53 — you must correct me if I get my dates wrong. In '53 I came into a lab. where, in fact, three different people — Arnon, myself and Mary Belle Allen — had suddenly decided, from different points of view, that chloroplasts must be able to make ATP. And, of course, the connection with the Calvin lab., then, was, of course, that there had been suggestions of ATP's involvement in CO₂ fixation and, from sort of general biochemical principles that must be the case. In Australia I had had some work done by a graduate student, Bob Smiley, and he, in fact, had demonstrated to me quite clearly how difficult it was to make mitochondria from leaves. We know that respiration is small in leaves, we know that photosynthesis can be big in leaves and, therefore, one has a strong feeling that maybe its these rotten chloroplasts that do the trick. From other points of view, Arnon and Mary Belle Allen were already coming along to this view. So we actually set out to discover this, which was a rather crazy thing to do, and we tried to make whole chloroplasts. This was different from the Calvin point of view.

VM: This was already in '54 when you came back?

BW: It would be the end of '54 — end of '53, beginning of '54.

- VM:** Can I take you back for a minute to the earlier period, the '48 to '50 period? What was the climate for photosynthesis like in Berkeley at the time? You said there was "interest". Were there seminars? Did people go to one another's seminars? Was the place abuzz with photosynthesis?
- BW:** No, it wasn't. It was more from my point of view an observation of Calvin doing things. We used to actually be amused to discover that reading Calvin papers, which were all group papers, of course, was interesting because a new compound would be popped in when they were trying to figure out how things cycled. As you are very well aware, this was a major problem and compound X would come in with a big question mark. The next paper there would be the same compound in without the question mark and in the third paper it would have disappeared. (*Laughter*)
- VM:** Without much evidence either way!
- BW:** That was amusement and that just represents that they were trying to find out how it worked.
- VM:** That was his style.
- BW:** That was the style. So we used to be amused by that and I may say it was something which in the Arnon group caused amusement and was completely alien to the way in which Arnon's thoughts would go. He wouldn't actually produce things like that. We are talking about Calvin here.
- VM:** Yes, of course.
- BW:** So in looking at this one, it was all a matter of it's all flying with obviously doing important things here and the "lollipop era", if I may call it that, was obviously a going concern. We were a little reserved, I think, because of the sort of biologists' view that, in fact, you couldn't actually have a standard *Scenedesmus*, or whatever it may be, because if you grew it under different conditions it might do different things. Biologists were very sensitive to this. I think we were always quite surprised, if you like and perhaps admired a little, Calvin's view that, in fact, the organism was an organism and it had all the necessary things to make CO₂ fixation go. He was going to use it just as a catalyst.
- VM:** That's very interesting you should say that. There weren't any biologists in Calvin's lab...
- BW:** No, not at all.
- VM:** ...as far as I can remember in the early days. The remarkable thing (*was*) that *he* felt that here was an organism that you could treat like a chemical and they set up, as you remember, continuous culture devices. He felt that you could then use it any day of the week and it would be the same as any other, which was certainly closer than it was for leaves but not, perhaps, totally close. It was not until the late fifties. I think, that questions began to arise of were these things really behaving the same way every day. And then we did some work to show that perhaps they weren't.
- BW:** As I say, I think there was quite a lot of suspicion...well, if you like — but not suspicion in a derogatory way — whether there might be something wrong with this one. In fact, if I can turn this around in a slightly different way, à la Arnon: when we were, in fact, starting to grow spinach as our material, we tried to standardise this as

much as possible. We have the myth that if you grow spinach in a container yea big by yea...

VM: You're describing something two or three feet by two or three feet...

BW: That's right...in a green house in water culture (hydroponics) — that's going back, if you like, to Soils and Plant Nutrition (*Editor: the department in which Arnon's lab. was located*) as it was then, that when it got to the stage after a certain number of weeks we could harvest it and we would actually say that the material was constant during the next two or three weeks. Now, I know that that's not true. But, in fact, it was a desirable and necessary myth to do anything. I think that's the same thing as Calvin did.

VM: Oh indeed. So, in 1948 Arnon's group was still in Nutrition?

BW: Yes, it was still very much in Nutrition. In fact, if I tell you that in 1954, when the first announcements were made of the CO₂ fixation and ATP formation by isolated chloroplasts, this actually was presented at an international congress in Paris and Arnon actually withdrew a paper on phosphorus, because he was still an expert in phosphorus from the plant nutrition point of view. In a sense there was very much "I'm still in plant nutrition". Arnon, in fact, got funding for work on manganese and molybdenum to continue in photosynthesis and CO₂ he regarded as an inorganic substrate, just the same as all the other things he had been interested in. That was the way he edged into this one.

VM: You were located in the building...what was it called; Plant Nutrition?

BW: No, it was called Soil and...the Life Sciences; it was in Life Sciences.

VM: Oh, you were actually...

BW: Actually in Life Sciences. The reason I'm forgetting this is it's now changed and they have actually moved into Hilgard Hall and then finally into a completely new building, the name of which I can't remember. Can you? Anyway, it's a new building.

VM: That's where Buchanan is?

BW: That's where Buchanan is.

VM: We saw him.

BW: If you can remember where he is.

VM: I can't. I know where it is but I can't remember what it's called.

BW: I'm not vaguely lost.

VM: So you were in Life Sciences? Fairly high up in the building?

BW: Floor 3. We had a little tiny lab. down on floor 1, the basement relatively speaking in that building, for a few of our things but mostly we were on floor 3.

VM: How big a group was it in the late forties?

- BW:** It was the three of us (*Whatley, Allen, Arnon*) with two or three other people coming and going.
- VM:** Students? Postdocs.?
- BW:** Postdocs. Students actually didn't flourish particularly well with Arnon because he had very positive ideas as to how they should operate. This, of course, tended to smother people.
- VM:** And he was the faculty member?
- BW:** Yes. I wasn't, no. I was actually in the College of Agriculture. It was a College of Agriculture group which, in fact, was nice because it was an 11 months appointment and we didn't have to look for funding for the remaining three (*months in the summer*).
- JW:** (*indecipherable*) during the build-up to Davis; this was part of the teaching that was left behind when starting up and half the people...
- BW:** That's a slightly later...Let me explain. It is simply that at the time, Soils and Plant Nutrition came together and were set up as a main group. Then, in fact, subsequently and in about 1953 or 4, maybe even just before that, the Biochemistry Department in Berkeley was founded. You may be surprised at how late that was.
- VM:** I am.
- BW:** Because, in fact, biochemistry used to be all over the place because it was being applied to particular projects. When that (*the Biochemistry Department*) was set up, most of the straight biochemists who were in Soils and Plant Nutrition migrated up to Biochemistry and subsequently they moved to Davis. The history of Davis is interesting because, of course, for the first two years students in Davis wanting to do scientific subjects did them in Berkeley and then they specialised in the Davis specialisations.
- VM:** This was Paul Stumpf and people like that?
- BW:** That's right — and Hassid, Barker, all these people. They'd actually all been in Plant Nutrition originally and then they moved on. So there was a hydro(*carbon?*) view and a metabolism view. And Eric Conn. So they all were temporarily in Biochemistry in Berkeley, having been in Plant Nutrition, and then they moved off.
- VM:** I mentioned this before to you, but let's push it a bit. What was the communication like among the biological sciences, or the people working on biological problems, around the campus? Because you were some hundreds of yards away from Calvin's lab., which was Chemistry- and Radiation Lab.-based. What sort of communication existed at that time?
- BW:** Mostly casual, very informal. And I think that if one wanted to go and find out things, that was easy to do. There was very little, as it were, official flag waving being done.
- VM:** Were there seminars which people attended?
- BW:** There were seminars within particular groups. In the Arnon group, for instance, we had our own household seminar. We used to try to get people to come to that from

time to time, particularly if there was some relevance, but it was mostly very much in-group. I think that would be fair. Roger Stanier, for instance, was a mover and he used to appear from time to time and would organise seminars. In fact, at one stage he actually did organise seminars formally in photosynthesis with a biochemical viewpoint and he used to bring along his interest in the microbes and Arnon used to bring his stuff, both in microbes and chloroplasts and Calvin used to come along with his group. And in fact these seminars very seldom lasted for more than maybe two or three simply because they could never get Arnon and Calvin to talk to each other.

VM: Which does, indeed, bring me to the Arnon-Calvin relationship. I know, of course, that there was antagonism between them of some sort but I don't have any very clear idea of what its origin was or what the nature of it was. What did it look like from you...? What do you think was the problem between them as far as you saw it?

BW: I think it was just the philosophy of the people. I'll comment, if you like, on Calvin but, in fact, the first problem, if you like, is that Arnon actually had to have an experiment in his hands to make any progress in thinking — even though it was blindingly obvious that this is what was going to happen, he could not accept that until he had done his own experiment. It's not that he disbelieved other people but he needed just to check out and he was only happy if there was an experiment. This sometimes led to silly difficulties, but never mind!

When he would talk to Calvin, Calvin used to have a much wider view and certain things to him would be obvious. He (*Calvin*) would say, in a particular situation, "this is the way it must be". Arnon would say "how do you know?", meaning "what's the experiment?" And that would be the end of the conversation. Because to Calvin there was a lot of background information coming along and he could predict this one with real certainty whereas Arnon actually had to have this experiment in his hand. There was an impasse.

SM: Calvin could be wrong occasionally.

BW: Indeed, not infrequently. So occasionally could Arnon although, in fact, because he was so experiment-based, it was a matter of interpretation rather than the experiment being wrong. So he wasn't picking up information from all the way over. He actually would collect information from all over and then, as it were, in his mind design an experiment that would convince him that the step forward could be made. Then, subsequently, that was fine; he could accept it all.

VM: So they just simply couldn't live with one another's approaches.

BW: No; I think their approaches were completely different. I mean, Arnon used to say "he thinks like a chemist". It's true, I suppose.

VM: I never actually heard Calvin say anything about Arnon. Calvin never mentioned Arnon.

BW: That's not quite true. I will tell you. There's a thing called the *Calvin cycle*, as you well know, and this, in fact, in Arnon's laboratory was referred to as the *reductive pentose cycle*. This is just because there was an antagonism, not expressed for any particular reason apart from the general philosophy, but Arnon used to say, quite cheerfully, "well, the Calvin cycle is fine, but, in fact, except for the ribulose biphosphate carboxylase it's the same as everybody else's reductive pentose system. Calvin doesn't seem to give any credit to that and we ought to give credit to it." It's

fair enough, and it just depends how you are going to operate. When it came to talking about particularly...excuse me, I've lost my way here. The reductive pentose cycle was fine and you could just see the different philosophies.

There was one very unfortunate occurrence and that was, I think, it was in *Physics Review* or something, one of these journals. After several years of Arnon doing his work on the actual isolated chloroplast CO₂ fixation of the ATP, there came a time when Calvin and a group actually published a review paper and in this review paper they state that the chloroplast can do the following things, so and son: they can make ATP and they can fix CO₂ and whatnot, and there is no quotation of this work from the Arnon group at all except that Arnon actually had to change his mind! So any time Arnon was wrong and changed his mind, he was quoted, whereas the implication is quite clearly that all this work is done in Calvin's lab. Now, I find this rather sad that it should be that way. Because, of course, from the scientific point of view it doesn't matter. But from the personality point of view it's really discouraging. I don't know whether you have come across that one before.

VM: Echoes of various sorts of relationships.

BW: That was a strange one.

VM: When you came back then in the early fifties, '53/'54, had things changed, had they got more entrenched?

BW: I didn't have enough information to allow me to make any serious comment on that one. I don't think that there could have been any very obvious difference in the relationships; I mean, I think they were all very curiously opposed, as it were. I do know that Roger Stanier tried quite hard to get them to get together because he, as a partial outsider, could say "well, look, we in microbiology are doing work and Arnon is doing work on microbiology and he is doing it on chloroplasts and Calvin is doing on algae, and we are all interested in photosynthesis. We have a powerhouse here, let's get together." Well, it never worked.

VM: Arnon's group, and indeed Stanier himself, were not actually working directly in the same area as Calvin were they?

BW: No.

VM: There was no obvious competition or treading on toes there.

BW: The only potential treading on toes was, in fact, from the point of view of chloroplasts will do the whole of photosynthesis. That is, in fact, a leap forward, as it were, a conceptual leap forward, which was in fact a significant jump simply because right up to '54 or so, '55, one of the problems was that many people believed that chloroplasts made something which was given to the mitochondria — and there was very good evidence for this one — and, in fact, Arnon is one of the people you can quote in favour of this, I mean in the early days. That conceptually was a difference.

Then you actually had the business of the chloroplast doing the whole thing. Of course, you can discount that because the rates of CO₂ fixation were so puny that they are not worth considering. Well, in fact, we had this interesting business of being able to get the rates up on a chlorophyll basis by the very justifiable cheating trick of

taking the chloroplast preparation, which was as whole as we could make it but we knew it was leaky. Then we said, "well, we'll throw away most of the green, which is doing the photochemistry, and put in the juice that is coming out of the chloroplast extract." Then, on a chlorophyll basis it was actually doing quite well. Philosophically speaking that's quite amusing because if, in fact, we had made chloroplasts, which people did later on, which would do CO₂ fixation at rates quite comparable — and that's to the Calvin lab. and Walker and people like that...

VM: ...that was already into the sixties.

BW: That was in the sixties. When that came along, that was fine. If we had made chloroplasts like that in the beginning we would not have been able to demonstrate ATP formation because intact chloroplasts won't do it. We, of course, went under the cheerful misapprehension that we had intact chloroplasts whereas we had in fact what are officially classified as *naked lamellar systems (indecipherable)*. Hence, they could actually see AMP or ADP if it was offered to them; they ate ADP like nobody's business. In a sense, we were trying to do something and we failed and, because we failed, we were able to demonstrate ATP. It's quite interesting and from a biochemical point of view, of course, there was a lot of stuff that was going at the time on CO₂ fixation in the CE system.

In fact, Arnon had a year's sabbatical at this time and I very foolishly thought "well, wouldn't it be nice if you put in different potential donors and acceptors into the system which were carbon intermediates to see what the products were and how quickly they came?" Because if you put them on the side leading up to CO₂ fixation, and not on the reductive side, you would actually have a different pattern: depending where the bottleneck was, you get different accumulations. I did a lot of these things which, in fact, were much better demonstrations in some ways of the Calvin cycle than anything you could do with the lollipop method.

VM: You were not using, were you, chromatography and radioactivity?

BW: Mm.

VM: The same sort of thing as...?

BW: Yes, exactly the same thing, sure; it there: why not use it?

VM: Yes, indeed.

BW: Mary Belle Allen and I and a gentleman called Rosenberg, Lawson (*spelling?*) Rosenberg, were all playing with this one. In fact, they concentrated more on the carbon side and I concentrated more on the phosphorus side. It was all done together. We end up with this work being done during Arnon's leave. The fact that he could confirm what Calvin was saying in a rather direct way, I would have thought would have amused him very much. But he actually didn't want it published.

VM: What happened? What was Arnon's reaction when Calvin won his Prize? Was he very upset about it?

BW: I think he was very upset and he called me in to tell me the news. He was at great pains to say that it was nice but not all that important. I'm sure he was very much cut up about this one. I think particularly because he had done his best to cultivate Europeans, particularly Warburg, to try to get himself known in this sort of position.

No, I think he was very much depressed by this because, of course, it meant that his chances of doing anything were out of the window. Because you're not going to get two Nobel Prizes for different aspects of the same problem.

VM: Even though Calvin and Arnon themselves may not have any close personal relationships, did people in his lab. know people in Calvin's lab. on a personal level?

BW: I think we used to interchange. Benson was fine; he was good. Of course, he left at a time when he was beginning to get on Calvin's nerves. In fact, I think that there was some sort of difficulty going on here that would be a problem. Of course, the difficulty was that Benson was very much the experimentalist there, getting in and doing things. When he went away he just left all that stuff behind him, which was OK.

JW: Rod Park?

BW: Yes, Rod Park.

VM: That was later.

BW: Yes.

VM: Park was '59-ish by the time he got there.

BW: I'll tell you a silly story about that, which is not actually apocryphal because I heard it from Rod Park. In fact, he brought down some ribulose biphosphate and we gave him some CE and we actually did the experiment to show what the products were in a test tube, being that part of the cycle. Which was fine. When he got back to Calvin's lab., Calvin said "I've drafted out a paper, just fill in the numbers", as it were. Rod Park's reaction to that was, as you might expect it to be. He said "no, I'm not going to give you that sort of information. I can just tell you that CO₂ fixation on ribulose biphosphate gives PGA labelled in the right place". And left it at that. That very interesting piece of work only got published as a footnote in an Arnon review because of the difficulties of not being able to communicate.

VM: You said just now...you used the initials *CEN*?

BW: *CE*, chloroplast extract.

VM: Yes, I'm sorry...

BW: No, no, that's fine. I have "CE" built into my brain!

VM: Whoever reads this in the future will not understand CE unless it is explained.

JW: Another thing that's very important and that is that Arnon didn't know how to talk to people. He talked to the children...he sort of petted them and talked to the children and he found it very difficult to get on with people generally. I think that was also part of the problem between him and other groups like Calvin's group; it was not just the Calvin one, it was generally.

- VM: I think that it was an impossible relationship, personality-wise. Arnon was a very reserved formalistic sort of person.
- BW: Yes, very formalistic.
- VM: I didn't know him well enough so I can't actually...
- JW: He was one of the kindest people imaginable, he really was...
- VM: But reserved.
- JW: ...but very reserved.
- VM: Calvin was very ebullient on the surface but had no patience at all for anybody with whom he disagreed. It was fine so long as things were going his way but terrible if they weren't or if there was any conflict. I can imagine that these two simply had totally the wrong sorts of personalities.
- SM: He also was not at ease socially...
- VM: Calvin?
- SM: That's right...not on a personal level.
- BW: Arnon wasn't either.
- SM: As a scientist, yes...
- VM: Can I remind you of something you've said before we actually started talking, about the way Calvin or Mrs. Calvin used to address you ?
- BW: No, no. Calvin himself obviously had the view that my name was "Arnon and Whatley"...
- VM: "Arnon *and* Whatley"?
- BW: ...and he had to be introduced to me each time, or I had to be introduced to him, rather, each time. "This is Whatley": that's fine. We would carry on whatever we were supposed to be talking about; it was usually only when I was visiting up there with other friends. This happened every time I went up there. He had to be introduced to me, he would never carry this on. Later on, when I was on sabbatical leave here and I was at Cambridge with Robin Hill, we went to a garden party, which The Royal Society was running, and I told Robin Hill that Calvin always requires me to be introduced to him. Robin Hill started to introduce me and Calvin almost fell upon me, "I know Whatley". That was fine and we got on perfectly well. When we got back to Berkeley about three months later, I had to be introduced to him again. This was part...I was contaminated, I'm afraid. (*Laughter*)
- VM: One point which has been made by lots of people, and I wonder what you think of it: people are very enamoured by the memory of the building, the old wooden shack in which the group was housed. You remember, it was a building with few partitions, it was an open lab. Many people feel that contributed a lot to the way the group ran. Did you get that impression, coming into it occasionally?

- BW:** I always thought that, in fact, the elegance of your surroundings are not particularly important as long as the tools are available. And they don't even have to be particularly complicated tools as long as they work every time you want them to work. Perhaps I didn't notice; perhaps I thought it was the way it should be. But I do know that when they did move into the Round House the suggestion was that the office in the middle occupied by Calvin would remain stationary and the rest would revolve around it! (*Laughter*)
- VM:** Your group in Arnon's outfit was presumably more conventional in structural design, was it?
- BW:** Just the long corridors...
- VM:** With rooms off it?
- BW:** ...with rooms off, that's right. We used to have one room in which we put (*momentos?*) and rubbish, and another room where we did the radioactivity, and another room for cold room and things like that, and another room where we put centrifuges.
- VM:** We had that when the old building was demolished and we all moved down to the Life Sciences Building basement for several years. But then, of course, it was exactly the same structure. People hated it; they'd been used to this open plan.
- BW:** Well, I can appreciate that. When the laboratory in Davis was designed to accommodate people who had started off in Plant Nutrition and then gone to Biochemistry, they actually got rid of all the corridors essentially, they had very wide corridors which in fact were the instrument rooms; then there were a few offices and labs. put on the sides.
- VM:** What was the funding like in Arnon's lab. in the early days, where did it come from?
- BW:** It came from the Navy, it came from molybdenum mining companies (manganese — same family), the Navy and then the National Science Foundation. Virtually all the money came at that particular time from the Navy to begin with because they were interested hypothetically in changing CO₂ to oxygen and this was all...
- VM:** This was for submarines, presumably?
- BW:** The cost of the light was (*indecipherable*).
- VM:** Yes.
- BW:** But, you know, this was the sort of thing...And the interest in manganese, of course, went on for quite a little time because of manganese and CO₂ fixation. Later on, rather more importantly although we didn't know very much about it at the time, that is that the four manganases were required to give one O₂ although we did actually have some clues on this one because we did good old plant nutrition analyses of isolated chloroplasts which would do this, that and the other and found, in fact, a lot of manganese there and very little in the rest of the plant.
- SM:** May I ask you a question going back a bit? You mentioned that Benson was getting on Calvin's nerves, or something.

BW: According to Benson, he left more or less following Mrs. Calvin saying that “if you go on like this, Calvin will have a heart attack”. That’s my view. I don’t know quite what the...I’m not privy, but I do know that statement was made and fairly soon after that Benson picked up a job.

VM: So there was some friction...

BW: ... there was some but I don’t know what it was, definitely. Which was perhaps surprising because Benson is such a happy-go-lucky gentleman. This is the problem between your scientific personality and your everyday.

VM: There are people who are very divided.

BW: Scientists have to be divided because they get so depressed.

VM: Well, you don’t sound *very* depressed!

BW: No, but I mean scientists assume that whatever they do is wrong as a working hypothesis but they also expect to be paid at the end of the month. You have two completely different scenarios!

VM: Just to go back to Arnon’s funding for a second: did you have to work hard for the funding?

BW: No. He did it...

VM: And it was adequate?

BW: ...it was adequate. He pointed out to us that the University was beneficent and gave funds for research which counted for approximately two weeks in the year. So, the other fifty weeks had to be funded from outside. So that’s what he did.

The science of the group, you asked me about earlier. We started off with a very small group and that lasted for a few years and, in fact, it was basically the three of us (*Arnon, Whatley and Allen*) with two more coming in and out again. Which surprised quite a lot of people when they discovered that a little group of three can actually accomplish quite a lot but, of course, a group of three which is going can often accomplish quite a lot.

VM: Were you teaching?

BW: No.

VM: Arnon was teaching, presumably.

BW: He had a little bit of teaching, not very much.

JW: (*Indecipherable*) had moved to Davis, actually.

BW: When he went on sabbatical leaves, I used to do his main course for him and he also used to do a graduate course which was actually on consulting the literature...used to be on plant nutrition but it turned into photosynthesis, with time, just to represent current interests!

Anyway, we started off with this group of three and then, later on, as interest moved to photosynthesis by bacteria and things of this nature, the group actually had to increase and grow to take that and so we had another group of three or four with Arnon sitting on the top. I suppose at the end there would be about eight or ten as a going concern.

VM: It was always very small, then.

BW: Always very small, yes. We usually tended to work experimentally in our own little bits and this is part of the trick, if you like, of using bits of equipment to work and this includes people. We used to compare notes and so our weekly seminar would be quite helpful to keep us informed. Somebody kept stock and said “how about so-and-so?” It would work very well from this point of view. The kitchen seminar, if you like, used to include such discussions as to what new bits of equipment are we going to need, or can we move this way or shall we go this way — the answer might be “no” or it might be “yes”. At one particularly fractious occasion, I was sufficiently annoyed to say to Arnon “what we need is a good decimal point placing machine”. He was actually off-balance and it took me about twenty minutes to convince him that this was a joke!

VM: He was that sombre, was he?

BW: Yes, he could be. When we had people from Europe, we would have this very interesting situation where they would get quite intense in discussing. They would say and would call each other “Mr.” Arnon would feel that you actually had to do something drastic, like parting the fighters. They weren’t fighting; they were just expressing themselves fine. In 1960, it’s getting on a bit now, we actually had a very good group there; in fact, six or seven were to leave within two years and they all became professors. It was obviously a going concern although they did their own thing.

VM: When did you leave?

JW: ’64.

BW: Yes, ’64, that’s right; I went to King’s College in London.

VM: What persuaded you to go back across the Atlantic?

BW: Basically an insecurity with the education of my children who were beginning to show some small signs of not being alienated but actually having to defend their parents for being different from the rest of the world.

JW: We didn’t like (*indecipherable*) schools for secondary education.

BW: Secondly, I had a phone call from King’s College saying “would you like to become”, that is an offer. Actually I had already had negotiations going on with Bennett-Clark, who had just moved to Norwich, and he actually was trying to get me a Royal Society professorship. This he was also coming along quite well; then it all fell by the wayside. I discovered afterwards that it fell by the wayside because King’s College had offered me a professorship — damn it!. I would have been very happy to have stayed in a non-teaching, non-administrative job.

VM: You stayed there for several years before coming to Oxford?

BW: I stayed there until '71. My sabbatical was to come to Oxford!

VM: You've never been back to Berkeley to work, have you?

BW: No.

VM: And you, presumably, have visited?

BW: I went one summer to Davis which was also almost the same thing in terms of meeting people that we had known before. That was actually, however, only a summer visit, not a long term.

VM: You've held the headship at Oxford for twenty years?

BW: Yes.

VM: Until '91?

BW: I thought I was going to escape that but my alter ego went to become Chancellor of Edinburgh University.

VM: Who was that?

BW: Smith, David Smith. He was going to take over for the last five years but he'd gone by then and he wasn't replaced for a while.

VM: Luck of the draw! Well, thank you very much. It has been very illuminating and you have told us all sorts of things that we haven't exactly heard from other people and we look forward to listening to it all and getting then typescript — and using it when we're writing up.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name FREDERICK ROBERT WHATLEY

Date of birth 26.i.24 Birthplace WILTON, WILTS. UK.

Father's full name FREDERICK NORMAN WHATLEY

Occupation COAL MERCHANT Birthplace WILTON

Mother's full name MAUD LOUISE (HARE) WHATLEY

Occupation HOUSEWIFE Birthplace WILTON

Your spouse JEAN MARGARET SMITH (BOWIE) WHATLEY

Occupation BIOLOGICAL RESEARCH Birthplace GLASGOW

Your children GILLIAN MARGARET (-> PAYNE)

JANICE BARBARA (-> COATES)

Where did you grow up? WILTON / SALISBURY England

Present community OXFORD

Education B.A. (CAMBRIDGE) - Biochemistry - Pt II

Ph.D (CAMBRIDGE) - PLANT BIOCHEMISTRY; M.A (OXFORD)

Occupation(s) 1948-64: Assistant/Associate Biochemist, Ag. (Berkeley)

1964-71 Prof. Botany, King's College, London; 1971-1991, Prof. Botany (Oxford)

Areas of expertise Photosynthesis; bioenergetics of *Paracoccus*;

GC/MS applied to bacterial metabolism with stable
isotopes; GC/MS analysis of poplar exudates (phenolic constituents)

Other interests or activities Since retirement - gardening,
travel, reading medieval history

Organizations in which you are active None; retired 1991 !

Chapter 46

GÉRARD MILHAUD

Paris

May 14th, 1997

VM = Vivian Moses; GM = Gérard Milhaud; VeM = Véra Milhaud;

SM = Sheila Moses

VM: This is a conversation with Gérard and Véra Milhaud in Paris on Wednesday the 14th of May, 1997 — just; it's getting towards midnight.

You were in Berkeley in Calvin's lab. in the mid-fifties. What was your background before you went there and how did it happen that you came to his lab.?

GM: My background was first to study chemistry and to become a chemical engineer at the ETH in Zürich. And then I started to study; at the same time I prepared my PhD thesis with Professor Karrer who was a Nobel Prize winner on the constitution and synthesis of vitamins A and E. I started to study medicine, the first years of medicine. Then I went to Geneva and continued to study medicine and, after the war finished, I came to Paris and spent my time between Geneva and Paris and finished my medical studies in both places. Then came the time to settle down and I managed to get a position at the Pasteur Institute in Paris and it was obvious to me that we had been staying way behind in terms of modern biochemistry and, even more, in the use of radioisotopes. Even so, the first use of radioactivity of iodine in biology has been made to study the thyroid function in animals by Robert Cori (*spelling?*) and Joliot-Curie.

So the question was where to go to study the preparation and the use of isotopes in biology and in man. Obviously California was very attracting, especially Berkeley, with the importance given to this type of study by John Lawrence. Even so, John Lawrence did not have, as a scientist, the best reputation. It became obvious to me that what was going on in the laboratory of Calvin should have been very interesting. So I managed to get a fellowship from the French government for a year and I needed to find a way to have the travel paid. At this time, the Fulbright funds had been available for travel but the first requisite was to speak English. I spoke very poorly English. Before I went to the examination, my wife spoke well English and I started to brush up my English. The result was very poor and the Fulbright people decided that they could not possibly give me the grant due to my poor English. I said to them, "but it's my only hope to improve in English, if you give it." They gave it and we came to Berkeley and, more precisely, to the Donner Laboratory where Dick Lemmon was in charge of chemistry with radioactive carbon.

VM: You had made this arrangement, of course, with them before you came?

GM: I made the arrangement that Calvin would accept us in his laboratory. As a matter of fact, in 1951 the first world biochemistry meeting after the war was held in Paris. Andy Benson, we did not know, attended the meeting and we happened to know him to discuss with him. He was at the time working in Norway. He decided to come back to Berkeley so we could make the arrangements to come to Berkeley and to work for Calvin with him. Except, the first thing to do when you arrive there is to make the synthesis with radioactive carbon of a compound. Calvin at the time assigned me the synthesis of C^{14} -labelled hydroxypyruvate because he thought maybe it could be involved in photosynthesis.

VM: Were you a skilled enough chemist at that time to do that?

GM: Yes. I was a skilled enough chemist to handle the small amount of chemistry due to the training at the place of Karrer. I had no use of radioactive handling but having been trained in the best of institutes in the field of microbiology, the type of technique you use for radioactive tracer is not so different from the attitude you have to have against microorganisms for contamination and so on. This was not a big gap. What was a big gap was to get so and so many millicuries, \$5,000 worth at the time, telling you if you fail, you can have another one, but this is going to be the last one!

VM: How did you travel to America?

GM: In a very difficult way because we had at the time...we went on a French boat which was, I think, removed from the German, called *Europa* or something like this and we had trouble getting the permission from the state to work in AEC. Because at this time you had all the question of Oppenheimer and they had been very anxious. At this time — I don't know if it was the same for you — when we eventually got the permission to come, it was almost in winter time and we had a very, very rough sea and, as you we did not travel first class, as you can imagine, it's a very bad memory for my wife and not a very good one for me. We arrived, she arrived very sick in New York and then we travelled by plane from New York to San Francisco. Then really it was a fantastic discovery to come to this place of the world and to this highly exciting place of Berkeley at the time it was.

VM: Did anyone meet you at the airport or you went by yourself to Berkeley?

GM: No. I think Andy Benson met us at the airport and brought us to the lab. Then we met with Calvin, (*whom*) we did not know, and he said "where are you going to stay tonight?" and we said we would go to the Durant Hotel or some place like this. He said "no, come to our home, we have a room for you and you will stay with us as long as you find a flat you would like to live in."

VM: How long did you stay there?

GM: A month.

VM: A month!

GM: A month, sir!

VM: You stayed in the main part of the house?

GM: Yes.

VM: He had a place below, didn't he?

GM: Yes.

VM: And you stayed in that?

GM: Yes. It was for us the most fascinating introduction to the scientific life of America to be with Calvin in his house.

VM: How so?

GM: Oh because he was very active, he was speaking all the time, telling you about his projects, making theories. It was very interesting to see this man acting and living the way it did.

VM: And he continued to do this when he'd left the lab. and was at home in the evening; he still continued to talk in the same way?

GM: For a while he collapsed because he had too severe a diet and so he was not very active. Then, after he had eaten something, he started again.

VM: Of course he had had a heart attack, serious heart attack some time before.

GM: Yes, yes. He was on diet.

VM: And lost a lot of weight.

GM: Due to the diet imposed on him by Gofman who started to study the relationship between cholesterol and protein and proposed a diet of index to foresee the chance of heart attacks in a person after ultracentrifugation of his lipoproteins. He was very strict about his diet and, of course, his diet did absolutely nothing to his brain and to his abilities and to his imagination.

VM: So he really collapsed when he came home?

GM: Then he started again.

VM: Did he keep this going until late in the evening?

GM: No. You see, he was staying up very early in the morning, I would say between five and five thirty, and he would be going to bed latest at ten probably slightly earlier.

VM: Did you see what did he do at 5:30 in the morning? He didn't go to the lab. that early, did he?

GM: No. I think he was reading and writing in his study.

VM: Did he have a big library?

GM: He had a big library but only on technical chemical/physical chemical or botanical books.

- VM: If you spent a month living in his house, you must have talked to him about many sorts of things? Was he interested, in your experience, in anything other than chemistry?
- GM: First of all, he was very interesting in telling you how he went to England on, I think, a Rockefeller Fellowship, the time he spent there, what he learned there. He was also very interesting in telling you about the relationship he had with Ernest Lawrence and the beginning of the first experiments with the cyclotrons. He spoke mostly of his scientific personal experience and about his personal pathway.
- VM: You didn't get a sense that he was a deeply cultured man, widely read man?
- GM: No. I would say, what was impressive to us, that a man of his stature was really addicted to science and was not interested in literature or art or music. This was the impression we got. (If you don't agree, you should say something, Véra.) He liked very much to be in his garden and to watch the growth of flowers and plants. His wife was very interested in gardening.
- VM: That's science again, isn't it?
- GM: Well, practical science. His house was a beautiful house made out of wood by a famous architect — I forgot the name.
- VM: (*Bernard*) Maybeck.
- GM: Maybeck. It was a Maybeck house surrounded by much green and plants and trees — and it was a small heaven, without any noise, and in this beautiful landscape. He did not have much view because he was located too low but within the place it was really very nice.
- VM: So you operated, in effect, from his house for a month. You presumably eventually found yourself a flat?
- VeM: We found before but he didn't let us go; he said it was not good enough. He wanted us to have something nice, good and we did not...
- GM: So we found a flat which was very convenient for us, in the right range of price, 2525 Durant Avenue which was very close to the campus. So about three or five minutes walking time to the lab., and we used to work late in the lab. The only problem was that at this time we needed a badge and armed guards had been watching us day and night in the lab., of course, going in and out.
- VM: So you went into the lab. then, you started to work directly with Andy at the beginning?
- GM: No, I started first not to work in the lab. you know but I started to work at the Donner Laboratory, which is a complete building, not as nice as the Old radiation Laboratory. There I worked with Dick Lemmon. Dick Lemmon spent a year in Zürich and had a good training in chemistry. I didn't speak English, as I told you, but I used to speak German. Dick Lemmon spoke also German and in this way we could communicate. Then I started slowly to learn not English but Californian language. My wife had big trouble coming from a strange part of Great Britain to understand local language, but I practically none it because I started from nothing.

VM: How long did you work in Donner, all the time you were there?

GM: No, only until I finished hydroxypyruvate. Then I had to move to the Old Radiation Laboratory and to feed this to algae with Andy Benson and Bassham.

VM: So then you learned all the tricks of the photosynthesis experiments.

GM: Yes...

VM: ...how to run chromatograms...

GM: Yes, and how to elute and to count. Absolutely.

VM: You said that your wife also played some part in this and you have a picture of her...

GM: We had a picture of here and I think we could find this again. I will have a look and I am sure we can find it. So there is one of the...Andy made some pictures and Alice — you did not know Alice?

VM: I did know Alice — yes.

GM: So Alice made also some pictures of this time for paper chromatogram.

VM: Whereabouts in the Old Radiation Lab. did you work? In the big lab. together with most of the other people?

GM: Big lab.

VM: Who were your colleagues there at the time?

GM: Clint Fuller the closest. Benson was not very far away, Bassham was not very far away and the fellow of New Zealand

VM: Alex Wilson.

GM: Alex Wilson. And who else were?

VeM: Ed Bennett.

GM: Ed Bennett but Ed Bennett was, I think, more in the Donner Lab., you know; he was running some experiments trying to understand...to disclose learning systems in rats.

VeM: There were also two technicians...

GM: *Il y a deux filles*; there have been two technicians, a blonde one and...

VeM: Lorel...

VM: Lorel Kay.

GM: Lorel Kay.

VeM: And her husband, Lorel Kay?

- GM: Yes, but you know, the one who was going to come later on, maybe this year; she wrote you a letter. You don't remember the name?
- VeM: Ann Hughes?
- GM: Ann Hughes.
- VM: Yes; good heavens! And socially: how did you spend your time socially?
- GM: Very well because, first of all, we knew very rapidly people within the lab. and outside of the lab.
- VeM: The people are very friendly.
- GM: First of all, due to the Pasteur Institute we came very quickly in connection with Roger Stanier and Gunther Stent and also with a strange fellow of the name of Chaikoff. Chaikoff told me "you made in your life a very big mistake; you should have been coming to me and not to Calvin.
- VM: So you actually had a big circle of acquaintances?
- GM: Yes, absolutely. Next thing, we also had good contacts with Evans (*Herbert Evans*), the one who discovered some...the role of some growth hormone; and Li (*Choh-Hao Li*) who was a chemist, who made the structure of growth hormones. Then we became acquainted with the French Consul in San Francisco who had a very large territory because he had Washington state, California and, I think, Texas or Nevada. For some reason he wanted to visit the Atomic Energy Plant and he couldn't do this. So we organised this for him. From then on every time, he had a reception in San Francisco he invited us.
- VM: So you had rather a good time, then, one way and another?
- GM: We had a very good time. In addition, I had far related relative, cousin, of the name of Darius Milhaud...
- VM: The composer?
- GM: Yes, who was teaching music at Mills College. Darius Milhaud was convinced that we could not possible stand for a long time American food so every Sunday he invited us for lunch to have a French meal at Mills College.
- VM: That was home from home. Did you socialise with people inside the lab.?
- GM: Oh, yes, of course. We had a lot of invitations with most of the people of the lab., especially with Andy Benson, within the lab., with Lemmon with Bennett. There was also another fellow which we liked who was working with Evans, the name was (*Don*) Van Dyke. Van Dyke is the fellow who isolated urine erythropoietin. He was working on ion kinetics. He was the first one — he didn't left a name — but he was very active and pleasant, Van Dyke. He's the one who demonstrated the effect of erythropoietin from urine using ion kinetics at the Donner Lab. and at the Donner Clinic.
- VM: Presumably coming from your Swiss background, you went on ski trips and mountaineering trips with the lab. people?

- GM: We went on mountain trips but we didn't do skiing. We went on camping and to camp trips to Lake Tahoe and also to Nevada and we went north to Fort Ross.
- VeM: We went to the desert with the Stents (?) for a very short time. They had a lot of rattlesnakes.
- GM: And mosquitoes.
- VM: After this work you did with hydroxypyruvate, did you then move into...you said you then moved into ORL.
- GM: Basically, hydroxypyruvate was rather disappointing.
- VM: But you looked at that from a photosynthesis point of view?
- GM: Yes. It was used but it was not the key of being an intermediate we would have been missing. So the next thing was that Melvin was convinced that thioctic acid would be an important compound in the energy conversion from light to chemistry. (Tom) Jukes who had isolated thioctic acid and produced a large amount of it at Lederle/Cyanamid came to the lab. and I was in charge of trying to disclose if thioctic acid would be involved by opening and closing the sulphur bond in the energy transfer. At the same time, thioctic acid could only be measured on paper chromatograms by bioassays so we made the bioassays with Clint (Fuller).
- VM: You spent several months or a year or more working on this problem?
- GM: Yes.
- VM: You published, presumably, papers...
- GM: Not many. I think we published two papers but not many really. Basically, we learned very well the techniques but the result had not been at the level of expectation so, therefore, I think that we rather published to paper to have a paper published but not really to disclose a major discovery.
- VM: How did you find Calvin's attitude to publishing? Was he careful to publish or did he like to publish quickly? What did you experience?
- GM: He liked to publish quickly. He was very impatient. He wanted things to be published as soon as they would be...We would rather to repeat this once to be sure; he said, "yes, but do it rapidly." He was very impatient.
- VM: So you think he was conscious of competition?
- GM: Yes, yes. I think he was very much conscious of competition and a person he profoundly disliked was Arnon.
- VM: Why was that, do you know?
- GM: Yes, because he thought Arnon may be closer than he was to the energy conversion. He was convinced that Arnon would be the guy who may take away from him the Nobel Prize.

- VM: Even at the time when you were there, which was maybe seven or eight years before he got the Nobel Prize, he was conscious of the possibility and was...
- GM: I would say the Nobel Prize was the goal of his life. He was very much pushing everything toward the Nobel Prize. At the same time, he was a very bright person. As soon as another topic, far away from photosynthesis, would be brought to him in a way he would like or in an intriguing way, he would say "go ahead. I'll find you the money and give you the things." He was very broadly open but his main objective was photosynthesis, path of carbon, Nobel Prize.
- VM: Do you think that he was really the intellectual leader and innovator of that photosynthesis work?
- GM: I think, first of all, he was able to put the people together, to collect the money, to have the space, to attract interest and to speak, not to the media — at the time it was not media — but to chemical meetings and to seminars. From then on, I'm not sure he would have been able to find by himself the C₇ sedoheptulose-ribulose pathway. I'm not sure. I think for this he needed people like Benson. But Benson would not have been able to promote enough, I think, the pathway of photosynthesis to come to Stockholm. He (*Benson*) would have made the discovery all right but he was too shy and not enough pretending, it seems to me.
- VM: It's very difficult to know what the fairness of this is because nowadays the name of Calvin is a better known name than the name of Benson.
- GM: First of all you see Benson, you know Benson now, but at the time he was a person having a lot of difficulties, personal difficulties. First of all, he was a conscientious objector. He got every day the communist paper which made him in a very bad position...
- VM: The communist newspaper?
- GM: ...at the University of California. McCarthy. Once he was called by the FBI to go San Francisco, not for having dinner at Fisherman's Wharf, and therefore personally he was rather shy and very much worried about what was left in the States for a person like him in terms of human rights. So he would never have been, I think, strong enough and enough extrovert to come to the goal assigned by Calvin for himself.
- SM: At that time, Calvin was completely apolitical, wasn't he? I don't think he had any...
- GM: Calvin was always apolitical. I think Calvin was never interested into politics and when Oppenheimer got trouble and moved during the night we have been there and had to escape and move, he had Russian friends and so on, Calvin did not care at all. He had no sympathy for Oppenheimer and it was not his job, basically. He was on good terms with John Lawrence. He did not really respect (*him*) because John was not so bright as Calvin was. He was very much in respect with Ernest Lawrence and with the chemists. He did not like very much (*Martin*) Kamen and he had great respect for a fellow you may not have know, who was the most impressive figure in microbiology of the west, Van Niel.

It happened that I met Van Niel several times and then Van Niel decided to spend a year at the Pasteur Institute with Lwoff. After a month, he said "I am fed up with this guy, I'm going to work with you."

VM: He said that to you?

GM: Ja. So he came to the lab. for a year and he is the one who isolated *Thiobacillus thiooxidans* from the earth of the Pasteur Institute. This micro-organism was isolated a long time ago at the Institute by a Russian of the name of Vinogradsky. He said "I know how to do it. You have to translate every day and I show how to do it and I go away for Christmas for two weeks and when I come back I want you to give me a new seed of this micro-organism." He comes back. We completely failed and we said "we are very sorry, we did not succeed." He said "OK, OK. I'll go back to the hospital and remove some earth." He brought this back and he said "you made a very big mistake." The mistake was the following. You had to boil the water to remove oxygen because, with traces of oxygen, the micro-organism would not grow. You had to have an Erlenmeyer flask, to boil, and when you cooled this, you had to let no air go into it; otherwise you are lost. He showed us how to do this but did not explain this so we did not notice it. We had air and had not been able to grow it. For him, it took three days to take this out of the earth and have a pure culture.

VM: Did you work on *Thiobacillus thiooxidans* yourself?

GM: Yes. Because this micro-organism needs only thiosulphate and carbon dioxide. Out of this it's making a new micro-organism. Therefore, it was obvious for us that we could see how specific the pathway of carbon for photosynthesis was or was not. Therefore, using the techniques we had been learning in California, it took us two weeks to demonstrate that we had the same pathway as in algae, with the C₅-C₇ sugar, and we could demonstrate the product relationship. Therefore it was absolutely sure that *Thiobacillus thiooxidans* were using the same pathway as algae.

VM: This was after you had come back from California.

GM: Yes.

VM: Back in Paris?

GM: Yes. This was due to the fact that we had access to the microorganisms through Van Niel.

VM: By that time here you had set up the right sort of facilities to do...

GM: When we came back the question was: will we be able to settle or not? The head of the French NIH, of the name of Gunillard (*spelling?*), was very helpful to me. He introduced me to the director of the Pasteur Institute and managed to get some grants and facilities from the French Atomic Commission. He was at that time head of a biologist same time of the French AEC. So we had plenty of possibilities.

VM: When you did this, and I seem to remember that the results showed that the same cycle existed in *Thiobacillus* as in the algae? I presume you told Calvin this, did you?

GM: We published this.

VM: How did he respond to that?

GM: Andy was enthusiastic and Calvin did not want to answer. He was very worried.

GM: Worried that what?

GM: By the fact that the path of carbon he had disclosed with Benson was not specific to photosynthesis. It was obvious that it was unrelated to photosynthesis. As soon as you could supply energy, direct chemical energy, into microorganisms, or light energy, the same system would be working as in yeast. He did not like this at all.

VM: Which year was this?

GM: '55.

VM: I see; at that stage...yes.

GM: Late enough so it did not harm him from his Nobel Prize.

VM: But he was worried, was he?

GM: But he didn't like it, according to Benson.

VM: You remember that, at the early days of the cycle work, people often called the cycle by both Benson and Calvin's names.

GM: It was Benson; yes, absolutely.

VM: And then later on, somehow, the Benson more-or-less got lost.

GM: Yes.

VM: It was just Calvin. Do you think this was an unfair effect for Benson?

GM: It was very unfair for Benson and, at the same time, it's easy to understand. Calvin was always asked to give lectures at large meetings or seminars or lectures at schools or universities. Benson only seldom. Therefore, what we would call now the media pressure was in favour of Calvin and not of Benson. It's very clear to me that the pathway was discovered by Benson. At the same time, it is Benson who had the idea of making paper chromatography, radioautography and, what we did not mention, destroying selectively the compounds to get the location of the radioactive carbon within the sugar...

VM: The degradation.

GM: The degradation.

VM: It sounds as if you feel that, in the end, Benson was really not sufficiently recognised.

GM: Absolutely.

VM: You feel that's the case.

GM: Absolutely. And, you see, what came later on, contrary to what did happen to Calvin, is very interesting for Benson. I managed, as a small recognition, to have him

awarded the *Doctor Honoris Causa* of the Sorbonne. On this occasion I had to present Benson and, if you remember, when he left Calvin he did a lot of work on phospholipids. Phospholipids became at this time, he did not demonstrate this but it became then a major components of all the membranes. And he (*Benson*) had clear ideas about this. Then he worked on arsenic and later on he worked on fish biology and on sulphur compounds in algae. I think he did not stop at photosynthesis but made a few discoveries; each of them would probably have been enough to make an average scientist well known. I think he had at least four scientific lives, in addition to the work he did with the cycle.

VM: It's getting very late and we must stop fairly soon. There are really two questions...

GM: No, but I have still another thing to tell you. Finish the question.

VM: No, no: I won't forget my question.

GM: You will forget your questions.

VM: Well, OK. The two questions I had to ask you I will tell you so that we can fit them in together. I would like to know of the building where everybody worked, Old Radiation Lab. itself. Some people have spoken about that being an important factor in the way the group operated in Berkeley; I'd like to see what you think about that. The second thing I'd like you to tell us, just quickly, what happened to you in the rest of your life after you went to Berkeley.

GM: So let's start with the building. I think the building was really a fantastic place and I think the disorganisation of the building was probably the most efficient organisation you could find. First of all, all the people had been working at the same place, or most of it, and there was a constant exchange among the people which you could not reach with walls and offices. It was very exciting to go upstairs and to see on the blackboard with chalk the date of the Nobel Prize awarded to Ernest Lawrence for the cyclotron (1939) and to see the place where the cyclotron was located. So I think this building was really exceptional. What came later on, was not as efficient, not to speak about the charm of this type of this type of wooden floored military building. When they started to destroy the building, I removed a key lock which I still have, which was the lock going into the cyclotron room.

VM: Well, you should keep it because there is very little left of that building: I think one door, apart from your lock, is all that remains. You agree that it was an important factor in the way the group operated. The second thing is, in a sense, what did you learn from that lab. and what did it do for you in the rest of your life?

GM: I learned, first of all, that if you have a goal, you should not save on money or on efforts. This was very alien to the habits we had in Switzerland, at Karrer's place or Ruczicka's. There was always limitation on material and money. At the same time, I think I learned that the most you speak to people and exchange ideas, you don't lose the property of the idea but you increase your chances of making progress. When I came back, as I told you, Roger Stanier wrote a letter to Monod telling him that he had known me and I should be working with him but I did not like to work on microorganisms so I said "no, I am interested in applying these type of techniques to medicine and to human disorders."

After the pathway of carbon in *thiooxidans*, I decided to go over to man and to study a very simple system — I thought it would be a very simple system — which is

calcium metabolism in man with radioactive calcium. First of all, what was needed was to use kinetic analysis and modelling to understand, or to try to understand, what would be the normal behaviour of calcium pools, and sub-calcium pools, in bone formation, bone destruction, urinary excretion, faecal excretion, and so on, and absorption. Then we could have a way to diagnose disorders, knowing what drugs would be doing to correct disorders. I thought this would last one year or two and then the next thing would be to understand more precisely diabetes. As a matter of fact, I practically never went to diabetes, stayed on bone and calcium metabolism. This led me understand what compound discovered in Boston by Hirsch and Munson, which called at the time *thiocalcitonin*, in heart would be doing and working. At this time, they had made an observation and the existence in rat thyroid of calcitonin. They studied immediately human thyroid and they published that thiocalcitonin was absent in man and probably some vestigial hormone or something vestigial without any importance.

I repeated what they did and found thiocalcitonin in human thyroid and disclosed what thiocalcitonin was doing, which was immediately to suppress bone destruction. At this time, when we came back from the States, I stopped in Boston and met the group working of metabolic bone diseases Albright. So I was aware of osteoporosis and other metabolic bone diseases and I understood that I had the way to treat osteoporosis by stopping bone destruction.

So I extracted calcitonin from the human thyroid; there is not enough to treat patients. Then I found that hog thyroid was very rich in thiocalcitonin so I made a preparation of hog thyroid which I injected to the patient with heart beating. I could demonstrate that thiocalcitonin was highly active in case of hypercalcaemia especially due to vitamin D intoxication, Badgett's disease of bone, which had no treatment at the time but aspirin, and osteoporosis and some exotic diseases. This was due to the fact, having made a model for calcium metabolism in man, it was possible to investigate and to establish a mode of action of calcitonin.

Then I went to the industry, telling them that I had the way to treat osteoporosis, and this was a complete failure. I visited Lilly people in Indianapolis. They had thiocalcitonin themselves. They sent me some for testing. They did not believe in the action. I went to Lederle/Cyanamid. They had some but they dropped it. I went to Merck, Rahway: same result. I went to Sandoz where a schoolmate of me, Bressanard (*spelling?*) was head of peptide chemistry, he said "no, it's extractive; we need a synthesis", and so on. I went to Ciba, with the same good result. Eventually, Roussel took it but they never believed in it. Even so, it is nowadays 1% of the world pharmaceutical market, the Roussel people are practically out. I think it was very important to have been there for what came later on.

VM: Have you spent the rest of your working life in Paris?

GM: Yes.

VM: In the university?

GM: First of all, when I came back to the Pasteur Institute, in '58 I became Professor of Medicine in Amiens. In '61 I came back to the School of Medicine in Paris. Then, in '66 I went to St. Antoine and from then I have stayed at the medical school...

VM: St. Antoine's a hospital?

GM: Yes...having a laboratory that was financed both by CNRS (*Centre National des Recherches Scientifique*) and CERN. In France there have been three or four laboratories which have a large supply of money due to this double label.

VM: And you are still continuing that work?

GM: I still continue that work in term to find a way to inject once monthly calcitonin instead of three times a week or daily.

VM: I think perhaps we should stop there because it's nearly the end of the tape. Let me simply thank you very much and tell you what a pleasure it has been for me to meet both of you after so many years of knowing your name. I hope it won't be the last time.

GM: Until the next time, soon I hope. If you come often here for your business, let us know and we can meet again.

VM: Thank you very much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name MILHAUD GERARD

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Your spouse PAMM VERA

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Areas of expertise NUCLEAR MEDICINE - ENDOCRINOLOGY -
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Other interests or activities THERAPY OF BONE & CALCIUM
METABOLISM DISORDERS - DISCOVERY OF CARCINO-
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Chapter 47

EDWIGE (INIA) TYSZKIEWICZ

Paris

May 16th, 1997

VM = Vivian Moses; ET = Edwige Tyszkiewicz; SM = Sheila Moses

VM: This is talking to Inia Tyszkiewicz in Paris on the 16th of May, 1997.

Can I start by asking you what your early career in science was and how did it lead you to Berkeley?

ET: I was working for a few years before in an agriculture institute in France and after a few years I was thinking that it would be very good to leave France to see something else. I got the opportunity to get a scholarship from the International Atomic Energy Commission who gave me a scholarship at first for one year and they prolonged for a second year but I had to leave before the end of the second year because I got the opportunity to have the work exactly in another good institution. So it was funny because I wrote to three labs. in the States and the first answer I got from Calvin's lab. Really, after eight days I got the answer — I wrote one day and the eighth day I got the answer. It was fantastic! So I didn't wait for the others. I did it; I applied to have the position as a post-doc. Even I didn't have...at that time I didn't have my doctorate but it was a position like it was a doctor's in Calvin's lab.

VM: You knew about Calvin's work?

ET: Of course, of course. I already worked with plants, not exactly in photosynthesis, and I was interested to see how was his lab. and what could I do there.

VM: Did you agree with him before you went what you would do or did you wait until you got there?

ET: No, I just wrote to him that I am interested in photophosphorylation. It was the only subject I submitted to him in the letter and he accepted. When I came to Berkeley, he sent me to Roderic Park to talk with him and ask about the project. I have to say that it took one year between the letter I got from Professor Calvin and my arriving there to Berkeley.

VM: When you first got there did you know anybody in California? Did you have any friends, any acquaintances?

ET: I had acquaintances because so many Polish people are all over and I had some cousins, Professor Lednietzki — he was a professor in California University. But

really, I didn't know anybody; I knew about them and I met them, of course, when I came but I didn't go there because of them.

VM: Where did you stay?

ET: In International House.

VM: So that was arranged before you went and you came straight there?

ET: Yes. I wrote to...I don't remember who told me but it is a good arrangement to be in this International House. You didn't live there?

VM: No, we were never there. We stayed in an apartment.

ET: I stayed there for six months, then I rent an apartment with two American girls, which was very convenient. All those places were so close to the lab. At that time it was still in the other...not in the round building but, how do you say?...Life Sciences Building.

VM: Yes. When you first knew that lab., the old wooden building had been destroyed...

ET: Yes.

VM: ...the old wooden building and you went into the Life Sciences Building.

ET: I didn't know this old one, which I would like to have known because...

VM: It was an interesting building.

ET: I think so.

SM: May I have the date when you arrived in Berkeley.

ET: I think it was December 15th, '59. I think it was that day: it was fantastic because it was a sunny day; I came to the lab. without any coat, students were lying on the...how to say...grass?

VM: On the lawns.

ET: Yes, which we don't see it in France, as you know. Probably not even...Yes, now you can see it but not at that time.

VM: So, California struck you as being a very different place from France.

ET: Oh, yes, very, very different. It was like, for me, the first days it was like a paradise, about the country. The lab. I didn't know the first day.

VM: Did you speak English well at that time?

ET: Very bad.

VM: So what did you do? How did you communicate with people?

ET: When I came to the lab. it was funny because I knew some English. But the Bassham presented me to the others and one of those women was Zofia Kasprzyk.

VM: Yes.

ET: You knew her?

VM: Of course.

ET: It was funny because I told her, "So I can speak to you Polish!" And she told me, "I was sure you were a Frenchwoman who was coming!" So we communicated with her in Polish. It helped me for a few weeks at least to communicate with the others.

VM: Are you still in touch with her?

ET: Yes. She is in Warsaw. She is retired. We write at least once a year.

VM: OK. I will take her address before I leave. And so when you went there did you start ...you said you talked first with Calvin and he directed you to Rod Park?

ET: Exactly.

VM: And what did you decide with Rod Park?

ET: That I would do phosphorylation. First I knew the different, how to say, how to extract chloroplast; I didn't know everything, those things, because I didn't work (*on them*). So it was for a few days or a few weeks I just make the acquaintance with different people to know how to use the material. And then I started to work with radioactive phosphate given to chloroplasts and see what happens.

VM: And did anything happen?

ET: Yes, you know at that time I and other people saw that before ATP, something else is synthesised. And at last I found something but it was a kind of artefact because we stopped the reaction with alcohol, which is a very bad thing because of phosphatase which is in chloroplasts we got methyl phosphate, ethyl phosphate and things like that which appeared on the chromatogram. After that, when I came back already in France, I saw what it was and somebody who took my work later saw it too and Ning wrote to me his experiments and I wrote to him my exactly the same conclusions.

VM: It is very interesting you should say that because when Metzner was in Berkeley...do you know Helmut Metzner...?

ET: Yes.

VM: ...when he was there around 1957 or something, he had exactly this problem and he thought he had something very interesting. Bassham was away at the time and when he came back he spent a whole year working on this problem and found it was methyl phosphate. I am surprised nobody told you about this.

ET: No.

VM: Very odd. Rod Park was not there at the time so maybe he didn't know but I should have thought Ning would have known.

- ET:** Before I left I spoke about that with Calvin but he didn't tell me about that too; I think he didn't know.
- VM:** I think Calvin was a little embarrassed by the whole business and maybe that's why he didn't tell you. Calvin was very excited when it first happened. You know how excited Calvin was with everything. And then he became very much less excited when he discovered what he'd got.
- ET:** I remember Calvin telephoned to Ning, "Shall we publish it or not?" And I was leaving Berkeley three days later and I think they decided not to, of course.
- VM:** So did you spend all your time in Berkeley working on photophosphorylation?
- ET:** Yes. My work was that but I spent a little time also with Martha Kirk to see how she was working with gases giving CO₂, phosphine and so on but it wasn't my work, my subject.
- VM:** Did you work in the big lab.?
- ET:** Yes.
- VM:** There were also small rooms but you had space in the big lab.?
- ET:** Yes, but this big lab. was separated, if you remember it was separation, and I worked for 6 months I think, I worked on Zofia Kasprzyk in front of me and beside was Martha Kirk. If you remember this division of the lab...
- VM:** I do slightly; It has been quite some time and I don't remember it very clearly.
- ET:** Do you remember the name of this Holland...it was a man from Holland who was working with oxygen at that time and he got some very interesting data?
- VM:** I remember a woman who worked with oxygen but she might have been before you. Her name was Ingrid Fogelström-Fineman, I think.
- ET:** No.
- VM:** Goran Claesson.
- ET:** Maybe.
- VM:** I will see. I have a list of everybody who was there. I will see if I can find a name for you and let you know. A Swede, you say he was, from Sweden
- ET:** Yes.
- VM:** I'll see if I can find a name. When I was in Berkeley last summer I saw pictures of you in ski clothes. Are you a keen skier or were you a keen skier at the time?
- ET:** You mean skiing?
- VM:** Skiing, yes.

ET: One or two weeks we went with Martha Kirk and with others, Heber, Ulrich Heber, for skiing, yes.

VM: Did you learn there or did you do it already? Did you already know how to do it?

ET: Yes, yes, I did. And one time we went to see the Olympic skiing. Do you remember it was an Olympiad in...?

VM: In Squaw Valley?

ET: In Squaw Valley.

VM: I remember there was such an occasion; I didn't go.

ET: It was the winter of '60/'61 — or '59...one of those two winters I spent in California.

VM: How much did you see of Calvin in the lab.? You worked with Rod but did you see Calvin much?

ET: Very little, very little. He didn't come really very much. We saw him every Friday morning, because one of us has to talk about the work, but we didn't see him very much. I think he was busy also with his courses. He was a Professor of Organic Chemistry.

VM: Yes. But presumably you talked a lot to the other people in the lab. in addition to Rod.

ET: Yes.

VM: Did you work closely with Rod Park? Did you see a lot of him?

ET: We spoke together but we didn't work, really, with that. One who was really very nice to everybody was Ning Pon. If somebody didn't know anything, Ning was always helping. You probably remember that, no?

VM: I remember that, yes. But then I have known Ning all the time; I see Ning quite often so I know him well. But I remember him from those early years. There was also a young man called Karl Lonberg, did you remember him?

ET: No. I worked a little with Heber, with Ulrich Heber, Roderic Park we were talking about phosphorylations (but not real work) and with Martha Kirk, Heber and Zangahar (*spelling?*) we worked together sometimes.

VM: So then you say you stayed in Berkeley for 17 months and then returned to a job that was offered to you in Saclay. Did you spend many years in Saclay?

ET: Twenty-eight years. The rest of my professional life.

VM: Did you every go back to Berkeley?

ET: Once in '68 and then I saw everybody. I saw the new lab., the new round building. It was funny.

VM: What did you think of that new round building?

- ET:** I thought it was very, very nice. Of course, I liked the Life Science Building, because it was...you know, spending so many months, it was nice. But I think that Calvin, if he could have this beautiful building and very comfortable for the lab., it was very good.
- VM:** Yes. You know, it was designed by the people who worked in the lab. Bassham was one of the chief designers — not the architecture but the general design, concept of the lab.
- ET:** I didn't know that. Did Calvin get it because he got the Nobel Prize?
- VM:** No, it was already in negotiation before then. I think the Nobel Prize came after the building was already agreed and I think perhaps even they had already started building, I can't remember, exactly, but it did not depend on his Nobel Prize.
- SM:** They had the funding but it was a question of, in any case, the old building was going to be destroyed and the group was growing and so they had to make other arrangements and this was happening already.
- ET:** The old building, the wood one?
- VM:** Yes. So when they moved out of the wooden building it was not clear whether they would get another building or when they would get it. They spent five years in Life Sciences, where you were, and during that period he was able to get money from different sources in order to build a new building. And then it was designed, and I was part of that design group — not technical design but concept design — and that must have started in about the end of '60, beginning of '61. I think it was a successful building but it was never quite the same as the old building.
- ET:** Sure.
- VM:** What do you remember...what did Berkeley do for you in your later career? What did you learn from Berkeley that stayed with you?
- ET:** How to say: I think that, maybe I shouldn't say that, but France, after the war, was quite poor. The labs. were poor. Of course, in '59 it was already fourteen years after the war but it was better than the very days after the war. But coming to Berkeley, seeing the lab. which was really full of plenty of everything. If I tell you that my chromatogram I made in France at that time was in this, how to say, those...
- VM:** ...cylinders.
- ET:** ...cylinders. I took my paper round, I sew it...
- VM:** ...with cotton, yes?
- ET:** Yes, with cotton, and then I put that and I saw if the chromatogram is going or not. So it was really very poor. We don't have very much possibilities. And then seeing such a lab. was really fantastic for me. When I came back to France seventeen months later, Saclay was really at the same level as the States, as the Berkeley lab. But I must say two things: Calvin's lab. got money also from Atomic Energy — United States Atomic Energy. Saclay was also atomic energy. Both had the most money came from that. It was my first approach to the scientific part. And second thing I should say that

I think two days after I went first in the lab., one of those girls, you would remember me the name I don't remember, say, "This evening there is a meeting of everybody at my home. You have to know people from the lab. and you are alone. Come, come and you will be like in a family." In France it wasn't like that at that time.

VM: It was more formal in France?

ET: Much more. I thought that people were really very, very good for those who were coming from outside.

VM: Friendly and open.

ET: Friendly and open.

VM: I think the lady who invited you might have been Ann Hughes.

ET: You know that she adopted a child?

VM: Yes. Was that the lady?

SM: Ann Hughes.

VM: That was the one.

SM: Which she later returned.

ET: It was awful. Is she still alive?

VM: She is still alive and she is now, I don't know, in her 70s...

SM: Nearly 80, I think.

VM: ...and she has a house high in the hills which she built (not with her own hands but she arranged for it) and she lives there with her cat and her dog and we get Christmas messages from her.

SM: And we see her when we go to Berkeley.

ET: Sometimes she invited me, later I mean, but this was my first invitation in Berkeley and I was very astonished that just we didn't know each other.

SM: She is very hospitable.

VM: Always, always has been like that.

ET: Not only she. I remember Martha Kirk who invited us. People were very friendly and France at that time it was much more...people invite after many months.

VM: Now is it more relaxed in France?

ET: Much more because French people travel and see, you know.

VM: So when you came back to Saclay in 1961, more or less, was the atmosphere still formal in the lab.?

ET: No, I would say it wasn't, it wasn't like that. Much less than when I was in the other institute. Maybe it was a much younger institute.

VM: Saclay?

ET: Yes. Saclay was finished about in '54, late '54, so people were young and not so formal.

VM: But even when you were in Berkeley in the late '50s Calvin, who was the oldest person there, was not yet fifty years old. He was born in 1911 so when you left he would have been fifty. Everybody in the Berkeley lab. was quite young. Calvin was the oldest and everybody else was younger than he was and from the beginning it was always very young. When he started the lab., in '45, he was only thirty-four years old, so it's quite young and the others were, of course, babies, almost.

ET: I think he was very young when he worked with Benson.

VM: Yes, indeed he was.

ET: And I think it was like we were, you know he felt like when he was young.

VM: Yes. Well, I would like to thank you very much for what you have remembered about it. It's very nice to see you again after all these years and I hope it won't be so long next time.

ET: I hope so too; I hope so too then we can meet in London.

VM: Indeed. Thank you.

Chapter 48

PETER MASSINI

Biezwil, Bern

May 21st, 1997

VM = Vivian Moses; PM = Peter Massini; KM = Katrina Massini

VM: This is talking to Peter and Katrina Massini in Biezwil in Switzerland on the 21st of May, 1997.

Can I start by asking you (and I'm sorry we are having to repeat this because the batteries ran out before) — can I ask you what was your early experience in science and how you came to go to Calvin's lab.?

PM: I studied chemistry in Basel, Switzerland and I made a thesis in physical chemistry. I had always liked to study biochemistry but I couldn't do that in Basel. When the thesis in physical chemistry came to an end, I wanted to go to the United States for a year. Hans Kuhn indicated to me the name of Dr. Calvin whom he had met while he was working with Linus Pauling in Pasadena. He (*Kuhn*) wrote to Dr. Calvin whether I could come with a grant from Switzerland. I should work in a field of physical chemistry in Berkeley.

VM: That's what you wrote to Calvin?

PM: That's what I wrote to Calvin. It was agreed. But when I finally came to Berkeley, Calvin asked me whether I did not want to work in the photosynthesis field, which was more interesting and much more "hot", and at that moment coming to an end and so I agreed. First he gave me a task to study, to make a kind of monochromator from old lenses of war equipment, army equipment; this was not work, not very interesting, but I engaged in photosynthesis, in the real path of carbon business. I don't know how. It just came so.

VM: So you learned the chromatograms and all...

PM: All that I learned. That was running well as a service by Paul Hayes and the other — Louisa Norris did algae and Paul Hayes mainly did work on kept the solvents and the papers and everything. And Al Bassham, I think, was responsible for the C¹⁴ preparations and everything. So it went so.

VM: You worked on different coloured lights in photosynthesis?

PM: No, this was left.

VM: I see: you stopped that?

- PM:** I stopped that work completely. I just worked together with...I can't even say what I did exactly. We were so together in this place.
- VM:** At the time you arrived in Berkeley, how developed was the photosynthesis theory? What did they understand and what did they not understand? I remember that the big experiment that you did, of course, was the light experiment, turning the light off. But when you...how did that experiment happen?
- PM:** I labelled the material for a certain time, a rather long time for what then was usual, a minute or several minutes. Then I switched out the light. Before and after switching out the light, I made samples, the lollipop, and made the chromatograms and everything. These compounds, first compounds were then in a state where the labelling all already at maximum. What happened after the switching out of the light was an index for the concentration of these compounds, the changes of concentration of the compounds other than changes of the specific activity. That was the point.
- VM:** Who thought of this experiment? Did you think of it? You don't remember?
- PM:** We discussed so much. Alex Wilson was always the first to discuss — he was right and did always so when he thought...
- VM:** Who was that?
- PM:** Alex Wilson. He put his...
- VM:** ...thumb in his mouth...
- PM:** ...when he (*was*) thinking. Then he said something very bright. Everybody, it was these discussions and I can't really remember who said what then was the first thing. It was the period when the idea of the circle, of the cycle was emerging, but it was not close, the cycle was not close.
- VM:** When you say "was emerging", do you mean in discussion...
- PM:** ...in discussion, yes.
- VM:** ...people thought this might be a possibility?
- PM:** We thought of the Krebs cycle, of course, and we, somebody, Calvin, probably not me — I'm not so bright, not so quick — said it could be a cycle. Bassham maybe, or all of them. We should find out that. Of course, when you stopped the process by switching off the light, that was the idea, the compounds which were just emerging would build up from there because their concentration would increase. Some compounds which were the first, depending on the light, would fall down; something like that. I don't remember it very clearly. And, of course, I can't say how it emerged.
- VM:** Of course, as a physical chemist you would have a familiarity with kinetics.
- PM:** Yes, indeed.
- VM:** So it was a suitable experiment.
- PM:** It was a key for me.

VM: You worked in ORL, all the time you were there...

PM: Yes.

VM: ...in the big lab.?

PM: In the big lab., yes. I had an office in a certain place to sit and one end. Working I was all over the place, of course, depending on what I did.

VM: I see that you are smiling when you remember this. Is it a pleasant memory for you?

PM: Oh yes. I think it was the best time, the best of the whole scientific career. It was the best time; it was such an intense cooperation and I never had that again.

VM: When you were doing your studies in Switzerland it was presumably not like that, not such a collaborative endeavour.

PM: No, no. We were two or three together, but not so intensely together. Each had his own thesis to make, his own work, and we just discussed together and had fun also outside the lab., but it was not so intense. And, it was much smaller, of course, as a group.

VM: This was, of course, the first time you had been in America?

PM: Yes.

VM: So you had the experience both of America and of this lab. at the same time.

PM: And California, of course, and the campus.

VM: You liked that campus?

PM: Yes.

VM: Did Calvin participate actively with you and the other people; did you see him very often?

PM: Daily, certainly daily. There was a time (*indecipherable*) on the table, white table and there was a time when Calvin he came, I don't remember, once or twice a day and everybody came together and let his newest papers, his newest chromatograms, films on the table and one discussed that.

VM: It was a very exciting time. I was there, of course, later than you but it was still happening.

PM: It was still happening.

VM: You stayed there a year?

PM: Almost, but not quite.

VM: That was your intention, to go for one year; you had a grant for that?

PM: I had an visitors visa — what was it called?

VM: Exchange visitors visa.

PM: And I couldn't stay longer; I had no more money!

VM: All of your work in Berkeley was on photosynthesis?

PM: Yes.

VM: The big experiment that you did, with turning the light off as you have discussed, you did that by yourself essentially?

PM I think so. I worked with the lollipop, I made papers, I made the films. I discussed it with other people. I made the work but I made not the experiment. The experiment we did several of us together.

VM: I seem to remember — I'm not quite sure about this — that the paper you produced was just your name and Calvin's, wasn't it?

PM: Yes. Yes, I think so. Steady State Path XX — it was in *Experientia*.

VM: Yes; something like that. Did you write the paper or did he write the paper?

PM: I think I wrote it and he corrected it. He did the introduction; I wrote the methodology and everything about the practical work. Together or so we wrote the discussion.

VM: Did you write papers with other people as well? Did you work in direct collaboration with other people on other projects?

PM: There?

VM: Yes, there.

PM: No, we had this one project. I don't remember...There was something special with that paper. It was not just a paper in the whole series. *Experientia* had asked Calvin to make a paper about photosynthesis and he offered me to make that paper together with him, to use it also as a kind...how do you call it?... acknowledging and so, what I did, for the granting...

VM: A report for the granting...

PM: A kind of report, sure, a form of a report.

VM: As far as I remember, that was one of the early papers, maybe the first paper, in which there was real experimental evidence for a cycle.

PM: I think so too, yes. I believe it was the first. It happened just at that time. I was collaborating with that and it happened that it came really good out in the paper.

VM: Later, of course, Alex Wilson did the experiment where he changed the carbon dioxide concentration.

PM: Yes.

VM: Were you there then?

PM: No, I didn't; we were away then.

VM: Because that was the other part...

PM: ...the corollary.

VM: ...the corollary of your experiment, yes.

PM: The idea was the same, I think, to look what happens when...to work with the first intermediates already saturated with C^{14} and look what happens when one of the sources is cut off.

VM: When you went to Berkeley you were already married and you had a baby, a young baby?

PM: Yes, three months.

VM: How did you travel to America?

PM: With the boat *Mauritania* to New York. I had an uncle in the neighbourhood, in the state New York; we stayed there a few days and then we went with a plane to San Francisco and there we were fetched by Paul Hayes and brought to the Claremont Hotel. Then we could stay a few days in Calvin's house, with Calvin...

VM: In the bottom of his house: he had a large room, I think, downstairs.

PM: Yes, we lived upstairs.

KM: We had a room...

PM: We lived upstairs. Eating the meals in the large room which was two stories high. In the meantime the housing department of the University looked for a house and drove us around all over Berkeley. No children wanted!

VM: No children wanted?

PM: Finally we found something in Francisco Street.

VM: In Francisco Street? Did you have a car?

PM: We bought one from the uncle of Martha Kirk...(indecipherable)...it was a second-hand car. He sold us an old pre-war Pontiac and said we should not drive harder than 50 otherwise it would break down!

VM: Having a child with you, did that make it difficult for you to mix with the other people socially?

KM: For me it was a bit. However, I could take the child with me to all of the parties or picnics, then I could take her always with me...also to Calvin's, even when it was very late in the evening. For myself, all day I was quite alone.

VM: Of course, you could only go out when you took the baby with you?

KM: Yes.

VM: So you were not able to work there or anything like that?

KM: No.

VM: Did you go with them to the mountains and places like that or was that not possible with the baby? They had ski parties and mountain...

PM: In my time there were no ski parties. We went to Tilden Park on picnics together with the group, and you were there too.

KM: Yes, picnics in the park with the baby and I could take part.

VM: How did you find living in America for the first time? What was the impression you had when you arrived?

KM: Wonderful climate!

PM: Roses in Christmas time.

VM: Basel is quite cold in the wintertime, I suppose.

PM: Yes, yes.

VM: Many people we have talked to have said how important they think the building was in Calvin's group, ORL. Did you think it was a major factor in the way the group operated?

PM: It was certainly very important that everything was open. It was actually just one room without doors. That was certainly important.

VM: So that people mixed together.

PM: Yes. The lunches together and everything. We had to take care of each other. If somebody used the lollipop and the other couldn't, then all these things...

VM: Was this different from what you had in Basel before you left?

PM: Yes, certainly. There I did my experiments myself, or maybe two of us, and we discussed it with the professor and another colleague and so; that was all. Later, I was alone with my...everyone had another theme, another task there.

VM: In Berkeley when you were here, were there many foreign visitors?

PM: Oh yes, quite a few.

VM: Other Swiss?

PM: No Swiss. Grant Buchanan. Then the Norwegian Per Christensen...

KM: and (*his wife*) Kari Christensen.

PM: Alex Wilson, Egbert Havinga.

KM: Yes, and another Swiss: Karl Wieland.

PM: But he was not in the group. He was physicist.

VM: There was another section of Calvin's group, you remember, in Donner Lab. Did you have contact with those people?

PM: Yes, rather open. Sometimes I had to do something there or to look for something. Of course, we met the people at the gatherings, Martha Kirk and Jack Lemmon...

VM: Dick Lemmon.

PM: Dick Lemmon, yes. Bert Tolbert was there, of course.

VM: And you remember the seminars on Friday morning?

PM: Yes.

VM: Did you give a seminar yourself?

PM: I don't remember.

VM: Calvin was very active in those seminars, I remember.

PM: Yes, I don't remember.

VM: You were there in '52/'53?

PM: '51/'52.

VM: And you came back to Switzerland?

PM: Yes.

VM: To the same lab.?

PM: I did not work any more. We came by Holland and I asked for a job there. We went to Switzerland but only to make applications and clean up and things. I didn't have a job in Switzerland. I didn't find a job. Ah yes, I would have found a job in physical chemistry but I wanted to do biochemistry, of course.

VM: Where did you go?

PM: To Philips in Eindhoven, (*indecipherable*) Philips' laboratories.

VM: You did biochemistry there?

PM: Yes, plant biochemistry, plant physiology and biochemistry.

VM: Using radioisotopes?

PM: I started with that. I did always work with radioisotopes.

VM: You were able to build on your Berkeley experience?

PM: Certainly. Not very successfully, but I did; partly I could use it.

VM: How long did you stay in Holland?

PM: Fourteen years.

VM: Then what happened?

PM: Then we looked for another place in Switzerland and found it in Bern, in the Theodor Kocher Institut, with a quite other field: blood platelets. With Philips it was not always photosynthesis, the first time only, but then I worked on, well...translocation of substances in plants. There, of course, C^{14} and other isotopes were important to a very good means. I did a little bit again in photosynthesis but on the other side, on the chlorophyll business...not very much. And then in '67 we came back to Switzerland, we wanted to go home for the kids and so on.

VM: Your children were then in high school, or nearly in high school?

PM: Yes. There I did work on blood platelets '53...

VM: '53?

PM: '83, of course '83, when I was retiring and came to Biezwil.

VM: Were you not living in Biezwil when you were working in Bern?

KM: No, we lived in Bern.

VM: I found you — the people in Berkeley had lost your address.

PM: I can imagine.

VM: Dick Lemmon thought you were probably in Switzerland, so I wrote to the Swiss Chemical Society, I thought perhaps they would know. But they said they didn't know who you were...that's what they told me. And so I looked in *Chemical Abstracts* and I found papers of yours from the early '80s, from the institute in Bern. I wrote to the director and somebody answered and told me where you were. That's how I found you; it was not very difficult. That's when I wrote to you some months ago to tell you that I was hoping to come to Switzerland and wanted to see you.

What sort of memories do you carry with you of the days in Berkeley. Did you ever go back to Berkeley?

PM: No.

VM: Neither of you ever went back?

PM: No.

VM: Thank you very much for all you've told us. One last question, perhaps: how do you look back on the time you spent in Berkeley?

PM: It was a very good time, really. I think I kind of, it looks more golden to me than it was golden but it was strong work, of course, intense work, but it was very lively. I never met again such an atmosphere of cooperation and collaboration in a laboratory, later or earlier. It was very unique for me. I think this was mainly because of Dr. Calvin's influence on the group.

VM: He was a good leader, was he?

PM: He was a very good leader, yes. He could bring out the things from his collaborators.

VM: Do you keep contact with anybody from that time?

PM: Not really, no, really not.

KM: Not in the long run.

PM: Not in the long run, no.

KM: In the beginning, yes.

VM: I'm very pleased that we found you, found you well, in this lovely place, and that we had an opportunity to come and talk to you, take your picture, see your old pictures. You will now go into posterity in the library in Berkeley. Your voice will be there for ever! OK, thank you very much.

PM: I thank you.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Dr. phil. Peter Leonhard Massini
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Areas of expertise Photosynthesis; Translocation in plants
Biochemistry of blood platelets.

Other interests or activities _____

Organizations in which you are active Retired since 1983
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Chapter 49

ULRICH (UTZ) BLASS

Basel

May 21st, 1997

VM = Vivian Moses; UB = Utz Blass; SM = Sheila Moses

VM: This is a conversation with Utz Blass in on the 21st of May, 1997.

Can I start by asking what your early career was in science and how did it take you to Berkeley?

UB: Well, I got to Zürich in the year 1945 and I could start...take studies, in chemistry there. After some years I was with Professor Karrer, made a PhD about ionon/iron (?) compounds which were used for carotenoid synthesis. When I had finished I asked Professor Karrer if I could get to America with a stipendium of himself. He was the leader of the stipendium fund in Switzerland. So he said "stay with me" and then you get it. I had to serve it off two years. I was doing some lectures for medical students and I had the laboratory for exercises for the medical students. In the same time I did some research work for carotenoid synthesis.

VM: Chemical synthesis?

UB: Chemical synthesis. In that last year, 1955, there was a huge congress in Zürich for chemistry and I was the press representative for Karrer and I was very proud of that. It was a unique experience. And then, at the end of the year I was asking again about America and he said "well, you can get your stipendium, but where do you want to go?" I asked for recommendations and he was recommending Professor Cram and one more. I said "what do you think about Calvin in Berkeley?" He said "oh, yes, that's fine." And so I got to Berkeley.

VM: You contacted Calvin and told him you wanted to come?

UB: Calvin was at that conference in Zürich and Karrer contacted him and so we met. Calvin was...oh, I don't know the word. He said "yes, come to us and I will order things".

VM: He encouraged you?

UB: Sure, he encouraged me very much so. Then, going to New York I took the boat and then the Greyhound to San Francisco and Berkeley. With that my stipendium was almost used up. So one of the first things I learned with Calvin was that he was very helpful in getting finances. I was staying at the I House and came into the Old Radiation Laboratory and Calvin was proposing I should do some work about lipids,

lipoid and lipid colorants. Of course, the photosynthesis way. My first introduction was in the library and then with Al Bassham who showed me all the tricks with development of the algae and introduction of the C¹⁴ knowledge.

VM: What time of year did you arrive in Berkeley?

UB: That was at Easter, 1956.

VM: Bassham went to Oxford, didn't he, later that year.

UB: No, next year. He was there as long as I was there. I got a contract to stay through September '57 and so did I and then I left. Work in that field was rather unusual for me.

VM: You were an organic chemist?

UB: I was an organic chemist. Usually I did synthesis.

VM: But you had no biochemical, special biochemical experience.

UB: I had no biochemical experience at all. But I had studied with Professor (*indecipherable*) in physiology and physiological chemistry. I was very interested in that. So by midsummer '56 I was trying to separate the colorants of algae by chromatography.

VM: Paper?

UB: First on nylon powder and later on calcium hydroxide and aside on paper. When the separation was rather good on paper, we started radioactive experiments extracting radioactive colorants from algae. That seemed to be all right but after some studies we saw that the separation was not good enough, there were fine coloured spots on the paper but the radioactive correspondence was also aside of it, so there must be uncoloured compounds which didn't belong to the column. This was my whole work for at least half a year to get rid of those uncoloured compounds. Finally, I didn't really succeed but it got quite good. There were some signs of the ways the radioactive carbon went into the colours. The colorants were chlorophyll a, chlorophyll b, carotenoids, α -carotene, β -carotene and some oxygen-containing carotenoids like xanthophyll and (*indecipherable*) and something like that..

VM: Were you using two-dimensional paper chromatography?

UB: Yes also. But it wasn't really necessary. The best separation was on two-dimensional chromatography and following the trail of the radioactive carbon going into the colorants. There seemed to be the way, first, to the oxygen-free carotenoids. They were getting radioactive almost at the same time, α - and β -carotene were always the same amount of radioactivity.

VM: This was in algae?

UB: In algae, *Chlorella* and *Scenedesmus*/.

VM: Using the standard lollipop procedures that they used?

- UB: Yes as Bassham showed me. We got the idea there must be an equilibrium between those two carotenoids. And for the other carotenoids there was ten times less radioactivity at the same exposure time in the molar amount of colorant than in the α/β carotenes. You got really the idea that the carbon is coming from the CO_2 of the surroundings, of the atmosphere for colorants first into α - and β -carotene, those colorants without oxygen, and then going on into oxygen-containing carotenoids. A similar picture was with the chlorophylls. If we compared the radioactive amount in molar amounts of chlorophyll a and b there was about at least double the time radioactivity in chlorophyll a than in b. This could mean that the chlorophyll b is built up afterwards from chlorophyll a. This work didn't really get to the last finish and Calvin gave it to Jan Anderson afterwards. She was a graduate student with him.
- VM: That was after you left; after you finished working with it in Berkeley?
- UB: No, even when I was there. He was trying other things with me. He wanted to see if I can find the way of radioactive iron into lipids, into colorants... We were trying very much, but I didn't succeed in finishing something. But it was very interesting work.
- VM: Most of the time when you were doing what you have just described, were you working alone, mainly alone?
- UB: Yes. I was working many nights. Well, I got help a lot from Al Bassham, from Ozzie with the algae and, of course, Jan Anderson, and Ning Pon was along even in the night. So it was a very interesting time. The atmosphere, you know, they were all together at the coffee table and it was overwhelming for me as a working atmosphere and an interesting scene to find out something really new.
- VM: When you had earlier worked for Karrer in Basel, was there that sort of atmosphere?
- UB: Not really. Students are coming and going together and it was a gay and nice atmosphere there in the Institute. The time I was in the Karrer private laboratory, there was Professor Euchster (*spelling?*), he was still in the laboratory and we both tried to do work together. Karrer was looking very often into the laboratory and I talked a lot when he came round.
- VM: He would come in informally and talk to people?
- UB: Yes; well, more so to the private laboratory than to the students.
- VM: I'm not sure I understand by what you mean by the "private lab.". Do you mean his research laboratory?
- UB: His research lab.; well he called it *privat Labor*. What do you want to know more?
- VM: Did you continue working in this area all the time you were in Berkeley? You said you went on to work on metals.
- UB: Yes, on radioactive iron, Fe^{59} , and then we made some studies with O^{18} . We put targets into the cyclotron and took out the isotope but even this was not really coming out as I would like to have it. I think Calvin was — what is it *enttäuscht*?
- VM: Disappointed.

- UB: Disappointed, yes, but he was so nice all the time, even if he obviously was disappointed that it didn't follow his ideas.
- VM: There was an experiment, or a set of experiments, I remember in which chromatograms were run and transferred to tantalum strips for irradiation in the cyclotron. Was that work you did?
- UB: Yes.
- VM: With Ingrid Fogelström-Fineman?
- UB: After she left I did it for some time. Then my time was over also.
- VM: Did you find anything out by using that?
- UB: No. There were some signs but I really forgot it. There was no paper about it, there was...well I had to do a report but I don't have it.
- VM: So you stayed until...
- UB: ...September '57.
- VM: So you were there a year and a half?
- UB: Exactly.
- VM: I remember during the time I overlapped with you that you were a very active member of the social activities of the group.
- UB: Was I?
- VM: You were a skier, a mountaineer, and things like that, outdoor type.
- UB: So outdoor...Almost every weekend I was off for the mountains for the seashore and for some excursion in society. I had to do some talks, conferences, for the Lions Club, for the Kiwanis and for churches...
- VM: Really?
- UB: ...yes, for the Lutheran Church, for a black church in Berkeley.
- VM: How did all this happen?
- UB: Well, I was asked for and I said "yes, I do it".
- VM: How did they know you?
- UB: I don't know. I think the Swiss were very well...they were welcome and I enjoyed it.
- VM: Did people invite you to their homes as well?
- UB: Yes, but much less. For instance, at Christmas I was invited to Alameda to Spanish-speaking people. There were some other arrangements like that. It was very nice but not often.

- VM: When you were ready to leave Berkeley, I remember you met your future wife while you were there but I don't remember whether you were married in Berkeley.
- UB: Not at all. Yes, all right. I got to know my wife there and we left together from Berkeley. We were driving along the Canadian border to New York but the car broke down near Chicago and from then on we went with another vehicle. In New York was my brother so it was quite natural that I got there.
- VM: When you left the US, you went back to Switzerland?
- UB: Yes.
- VM: Did you have a job waiting for you?
- UB: I had a proposal to go to Sandoz and see them. But I didn't just (*do*); I went to Britain first, Liverpool and London, and then to Holland to meet Nel (Prins-van der Meulen) and then to Germany to the Bayer company to have a look at Agfa-Gevaert, and two other companies in the Ruhr-Gebiet.. Then to Switzerland. They were proposing to me that I should visit physicians and explain medicals. So, I was doing an SOS call to Karrer and he said "well, I will ask in Sandoz if they don't have something else". So I got to the dyestuff company, dyestuff section and was doing dyestuff research there at Sandoz for 28 years, always in the laboratory.
- VM: Until you retired from there
- UB: Until I got to retire.
- VM: When you went back and began to work for them, what sort of a lab. was that you worked in? Was it also the Berkeley type of lab. or was it much more compartmentalised into small units?
- UB: It was a commercial lab. but it was modern and there was everything in it I needed to start. Then I was rather free to buy and to have everything I wanted, not too much, but I had to look for what I needed for colour research.
- VM: I presume it was your choice to stay in the lab. the whole of your whole career there, not to become an office manager or something of that sort?
- UB: Yes. So I didn't want to go for medical explanations and then I got a lab.
- VM: Did you go back to America? You visited the people in Berkeley on more than one occasion
- UB: Thirty years later.
- VM: You didn't go back for thirty years?
- UB: Only at that meeting with Calvin in '89, I went back to Berkeley. The lab. wasn't there anymore.
- VM: Indeed it wasn't.

- UB: There was a completely new lab. building and everything looked very nice and it was fun to see the old colleagues, many friends and, of course, it was very nice to find Calvin there again. It was so short a visit and cold. It was never so cold in San Francisco before! We were invited to stay with Mel Look for that meeting and that was very nice of family also. I was so sad that he has died also in the last year.
- VM: You sound as though you thought the building, ORL, was a very satisfactory place to work.
- UB: Yes.
- VM: What do you think was good about that building?
- UB: Of course it was specially built for these photosynthesis experiments but there was everything which they needed; and, what for me was so nice, was the atmosphere with all the people. The secretary and Paul Hayes who was attending to everything what was necessary. I got many friends there.
- VM: When you went back to see the round building — as you know the wooden building was demolished to make room for the chemistry department, the new chemistry department — when you saw the round building, do you think this was a reasonable success in recapturing the flavour of the old or was that really impossible to do?
- UB: Well, I didn't see the old flavour.
- VM: You saw the flavour of ORL.
- UB: Yes.
- VM: But in the new building, you didn't sense what the building was like when you visited it?
- UB: I knew that Calvin was very proud about it and I got some papers about it before. I was prepared to see it. But I think it was so big and there were other people now that I couldn't find the old atmosphere.
- VM: Of course, you are right. The new building was not developed organically the way the old building was. It was built and people moved in by design, and so on. That was one reason why it was different. Another was that it was much bigger and did many more different things.
- UB: Yes, that's the point. Calvin had so many more interests in the meantime and so there were more sections in it.
- VM: The old building was much more focused on one very exciting topic and essentially everybody was working in the same field.
- UB: Exciting and very successful. I think for the two of us it was so fine that Calvin got the Nobel Prize.
- VM: Oh absolutely, yes. I think most people who were there during that period, up to 1960, approximately — a very exciting time, particularly in ORL — have thought of it as one of the most exciting professional times of their life.

- UB: Clearly; for me that's clear. I had exciting times with Karrer also, of course, but that's a completely other atmosphere.
- VM: Another big factor was the internationalism of the lab. in Berkeley, such a high proportion of visitors from other countries, or students from other countries. It was unusual, I think. Certainly Europe at that time did not have as many foreign students at that time, foreign visitors.
- UB: You are right. But I want to say that the Swiss universities had always very international company of students...and professors. But it was much more friendly in Berkeley, I think.
- VM: Is the atmosphere in Swiss universities, or was it then, rather a formal atmosphere?
- UB: Yes and no. We as students tried to get rid of it to be formal, and even the professors also.
- VM: One of the things that struck me about Calvin's lab. was, with the exception of Calvin himself, everybody used first names. The students to the scientists who were much more experienced, still used first names. It was only Calvin that by some common consent everybody called "Dr. Calvin", although he didn't ask for it.
- UB: I asked for it. I asked him and he said call me "Dr. Calvin".
- VM: He did, did he! That's interesting. Because some years after you left, in the middle sixties, people already began to call him "Melvin". By then, of course, everybody was a bit older and we felt, and I was among them, we had known him so long that it was become excessively formal and we started...some of us started to call him Melvin. Not those people who had been his students —Bassham and Bennett and Lemmon, in particular, were much more hesitant. I don't think Calvin even noticed that we changed.
- UB: Between friends, we called him Melvin. But I don't think I used the first name toward Dick Lemmon or...
- VM: You did not?
- AB: No. Speaking with Dick about Calvin about Calvin, I wouldn't name him Melvin.
- VM: No, that's true. But you would, of course, call Dick by his first name?
- UB: Yes, of course.
- VM: Everybody except with Calvin. It was an interesting way the way that developed. Later it broke down: at the very end, Lemmon and Bassham and Bennett also called him Melvin, but that was much later. I don't know exactly when it started but in recent years, in the few years before he died, they certainly did that. They were the last ones to change. And Marilyn Taylor (you remember Marilyn Taylor?) *never* called him Melvin. We saw her last summer in Berkeley and she was still calling him Dr. Calvin. He seemed to regard that as normal and she did too. So that was their arrangement and they were happy with it. Aside from that, there was very little hierarchy in the lab. and people did not use any formality between one another.

- UB: Not at all. For me the atmosphere in California was so unexpected that I came as a stranger and was welcome everywhere. I couldn't think of an atmosphere like that in Switzerland. A little more so now, but then — impossible.
- VM: I think people were very open and very welcoming generally at that period. A factor, I'm sure, which contributed was the fact that we the foreigners were relative novelties in California at that time. It was fairly soon after the war and people were still excited by this movement of people.
- UB: There were so many people around who were not Californians and they knew the hard times before when they came and so we were welcome to those people.
- VM: As we told you, we felt very much that it would be...we didn't want to lose all sight of this marvellous time in our lives and the only way we could think of doing it was to try and record some of the impressions that people had, what it was like for them, and try to build it into some sort of permanent record. So that's what we are going to do with this thing.
- UB: I have another kind of record of the time, about a thousand dia pictures, and I still like them.
- VM: We also have them but I must confess it's been a long time since we have looked at them, but while we were there last year we spent a lot of time looking through Marilyn's collection of pictures. You were one of the people who was recorded there.
- SM: You mentioned some of your social arrangements that you went away almost every weekend with people from the group and so on, and you also mentioned your relationships through the churches, through lecturing and so on; did you visit other people, members of the group in their homes, do you remember that?
- UB: Well, for instance, Karl Lonberg...who more? Oh yes Ozzie and, of course, Calvin and Dick Lemmon.
- SM: So socially you enjoyed it and you found people were friendly...you seemed to be very busy.
- UB: Yes, I was. I was busy also
- SM: You were working...
- AB: Of course, there are more but my memory.
- VM: Well, I'm afraid it was a long time ago. Anyway, we would like to thank you very much for meeting us again and talking to us in this way. Your voice will now be saved for posterity.
- UB: Thank you very much that you are coming here, such a long way.
- VM: It's a pleasure.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Ulrich Blass
Date of birth 15. 11. 1924 Birthplace Leipzig / Germany
Father's full name Curt Blass
Occupation Dr. jur., Librarian Birthplace " "
Mother's full name Johanna Blass, born Miller
Occupation Housewife Birthplace Biberist, Switzerland
Your spouse Inga-Lisa Blass, born Paulsson
Occupation Teacher, Housewife Birthplace Björnskulla, Sweden
Your children Hanny Blass, * 30. 11. 60
Per Blass, * 10. 3. 63
Where did you grow up? Berlin, Germany
Present community Reinach, near Basel, Switzerland
Education Humanistisches Gymnasium Berlin-Steglitz
Chemistry studies in Zürich, Ph.D.
Occupation(s) Colour research in Basel at Sandoz AG
" "
Areas of expertise " "
" "
Other interests or activities Botany, Archaeology
Mountain trips
" "
Organizations in which you are active " "

Reinach, 21. 5. 97

Ulrich Blass

Chapter 50

LUISE STANGE

Göttingen

May 23rd, 1997

VM = Vivian Moses; LS = Luise Stange; SM = Sheila Moses

VM: This is a conversation with Luise Stange in Göttingen on Friday, the 23rd of May, 1997.

Luise: what was your original science education and what led you to go to Berkeley when you went there?

LS: Well I was a student of biology and chemistry at first in Göttingen and then later in Freiburg-im-Breisgau. I did my PhD in Freiburg with Professor Oehrkers who was a geneticist. I was interested in genetics and also in the physiology of development. For a short time after my promotion I had a job in industrial research with sugar beets but I was not very content. I was offered a place in Cologne at the *Institut der Entwicklungsphysiologie*...

VM: That's Developmental Physiology.

LS: Developmental Physiology...and there I started my *Habitation* work and this was on regeneration in plants, especially the question, the following question interested me: what happens in the soil which changes from nature stage and return to the cell cycle activities? Of course, to have a new cell division, it's very important that DNA can be synthesised, which is not the case in nature cells. Therefore, I tried to get more information about nucleic acid synthesis in these cells. For this I used labelling with P^{32} but I found out immediately that it isn't very specific because so many cell compounds become labelled with P^{32} .

At the time I was offered a grant from the German Science Ministry because they wanted after the war young scientists to be educated in the United States, especially in radioisotope techniques. Therefore, I wrote at first to Andrew Benson because I had read his papers. He answered that the best place would be to go to Dr. Calvin's lab. to Berkeley. I wrote to Dr. Calvin and I was very glad that he accepted me. Actually, he wrote to me that he was interested in my work but that we should do something of mutual interest. He was working with *Chlorella* and when I came to Berkeley we decided that I should try to grow *Chlorella* synchronously which was just developed at the time by a Japanese group and also a German group. My task was at first to cultivate *Chlorella* in synchronous cultures.

VM: Can I go back to your approach. How did you travel to Berkeley?

- LS: At that time I took a plane, still with propeller, from Paris to New York, it was Air France. Then in New York I had to change. There was just a holiday, I think it was Labor Day...can this be in September?...so there was a delay and I had to stay there for one night. It was very interesting to see New York.
- VM: This was the first time you had been in America?
- LS: This was the first time I came to New York, the first time and I had to phone my landlady I would arrive a day later so I came later. Then I flew to San Francisco and I didn't know how to come. They told me that you have to change immediately and get a helicopter to go to Oakland. In a rush I came to Oakland and took a taxi and arrived during the night at 2511 Hearst Avenue.
- VM: You had already arranged this apartment?
- LS: Yes.
- VM: Had you done it or had they done it from the lab.?
- LS: I can't remember this; probably I got a suggestion from the lab. 2511 Hearst was just at the border of the campus.
- VM: So, you arrived at your apartment in the middle of the night?
- LS: The landlady was still awake or waiting for me and was very nice and helpful.
- VM: You had things in the apartment to live? You had furniture and bed linens?
- LS: Yes.
- VM: So the next morning, what did you do?
- LS: I went to the lab. and there I met at first I met, Dr. Bennett, Dr. Ed Bennett and I was astonished seeing him. He was very friendly and said "Hello, Luise, please come in". He immediately put his legs on the table, which is not usual in Germany. I was a little bit astonished but I didn't say anything, of course. Then he wanted to know what I intended to do. At first he hoped, apparently, that I could work together with him on his rats but I said no, I didn't want to work with rats, I wanted to work with *Chlorella*. Pretty soon I came then to the Old Radiation Laboratory and a lab. space was given to me. And I almost think that Dr. Vivian Moses was the next one — no, I think at first it was Ozzie Holm-Hansen; he was at first responsible to help me at the beginning. And then in a very dark lab. upstairs in this Old Radiation Laboratory and very close to my desk was the desk of my later friend, Erminio Lombardi, who was always singing Italian opera because he had done this in Milano before. And I remember downstairs there was Ning Pon and he had wonderful records playing during the night because We three worked very often during the night Ning Pon, Erminio Lombardi and me. Americans always finished their work at 4:30, I think, about this, while we intended to get as much work done as we could in a short time.
- VM: Your first scientific contact on the algae was really with Ozzie, but had you met Calvin yet at this point?
- LS: Yes, I had met him, probably at the second day or so, and he said "hello, Luise" (this was unusual for me, too, the first name, which is not used in Germany). He was very

friendly but we didn't have a very long speech at that time. We didn't have a very long discussion but also when I had set up the tube with the algae — he was interested to see the method for the synchronous culture.

VM: So you started to talk science with Ozzie Holm-Hansen.

LS: Yes. And pretty soon Dr. Vivian Moses joined us. I think he had the same room like Ozzie Holm-Hansen./

VM: Yes, Ozzie and I shared an office.

LS: I was very thankful because I had not education for a biochemist or biologist and so learned many small things from Dr. Moses, Vivian. Remember, for instance, shaking the beaker. I had to put it on the lab. desk so that it was shaken horizontally and not in the hand, not hand-shaken. I remember this very well.

VM: I think I have forgotten that!

LS: Later in my own lab. I always remember that if I had a beaker in my hand what Dr. Vivian Moses had told me at that time.

VM: What was it that you agreed with Ozzie, and perhaps with me, to work on at the time?

LS: I was interested in nucleic acids and so, of course, my idea was to follow nucleic acid synthesis through the cell cycle. But to get these nucleic acids labelled is not difficult because the methods were available in the laboratory. But to isolate these nucleic acids from other material, it was difficult. There I had close cooperation with Vivian Moses and we together worked out a method to fractionate this material. At first we could some of extract the soluble material, in which we were not very much interested, only the radioactivity of the whole fraction, and the insoluble material...this had to be fractionated further further. We decided to do this by enzyme digestion. Actually, the results of this work has been published in *Biochimica et Biophysica Acta* together with Dr. Moses and Dr. Calvin.

VM: What you were interested in was to look at nucleic acid synthesis in synchronised *Chlorella*.

LS: Right.

VM: Were you interested in that? I think the lab. had no synchronised *Chlorella* at that time.

LS: I had to synchronise these *Chlorella* cells and there I could rely on the papers of the Japanese fellows, Tamiya and co-workers, and then, in Göttingen and Pliser (*spelling?*) and co-workers. It is done by special light-dark changes and nutritional changes but, of course, we had to check if the culture was really synchronous. We did this by microscopic analysis of the cell size. Of course, at the beginning of the cell cycle, the cells have to be very small and then, at the end of the cell cycle before they divide, they had to be very big. So they had to be uniform. This was hard work: I had to do every day to check the cell situation in my cultures. It worked out and it was published also in another paper, not the fractionation paper (this is only the method), but another paper "Short-time $C^{14}O_2$ incorporation, with synchronously-growing *Chlorella*".

- VM:** How did you set up the synchronous cultures? What did you use? What did you use to grow the cells?
- LS:** The nutrient medium?
- VM:** Well, did you use the continuous culture tubes in which the other algae were growing?
- LS:** Yes.
- VM:** You used one of those tubes?
- LS:** Yes.
- VM:** And you had a light-dark regime and a nutritional regime?
- LS:** Yes, yes.
- VM:** And the cells you measured through the microscope, visually, counting cells, estimating cell sizes, every day. How often did you have to make the measurements?
- LS:** I checked it every 12 hours because the cell cycle was about 36 hours so I had 4 measurements during the cell cycle. This was enough to judge about the synchrony of the culture.
- VM:** What do you remember about the way you worked in the lab.? Did you tend to work by yourself? Did you work with other people? Did you collaborate in experiments? How did it go?
- LS:** Actually, with my project I was rather alone because until then nobody was interested in labelling nucleic acids by $C^{14}O_2$. But with the technical things I got help from every side. Also I remember Pat Smith, she was in charge of the algae culture, and she helped me quite a little bit with the preparation of the nutrient solutions. I learned many practical things. Of course, in the analysis of the material I learned from Vivian Moses how to make hydrolysis and sealed tubes — I had never done this before — and many other things, many small things which are very important in lab. work. I got support from everybody. When I asked somebody, I got help from all sides. But of course, the people were more interested in photosynthesis and not in cell cycle analysis, although Dr. Calvin was very interested in the project and I had to give a seminar and prepared myself very carefully in the library about the cell cycle. This theoretical work has helped me later very much in my work at home because, until the end of my career, I was interested in the cell cycle and its regulation because it is a very complex process and it's very closely related to the process of differentiation. So it was a very fruitful time from the theoretical point of view for me.
- VM:** When you worked in the lab. there, you participated, presumably, in the discussions which were going on all the time in the lab. Did other people look at your chromatograms and your results, and discuss with you what was going on?
- LS:** Yes. At first, Dr. Bennett was also interested. One of the papers on short-time incubation. This was not so close to my original interest, because we didn't analyse the nucleic acid synthesis in this paper, and he was interested because I think he was advised to help me — he was the first person I met there — and I learned from him

quite a little bit also. But most people there were interested in photosynthesis, of course.

VM: So you did feel a bit to one side of those people?

LS: Yes. Although not without interest of other side. I have to mention still that I brought with me my experimental plant which is a liverwort *Viyella* (*spelling?*); it is a single cell layer so you can look into the cells directly in this plant. Dr. Calvin allowed me to do incorporation experiments with this plant too. I think because of some experience with German workers he didn't want me to do only my own project. He wanted to integrate me into the group. I understood this quite well.

VM: Which, of course, he did.

LS: He did.

VM: Was this type of laboratory very different from your experience in Germany before you came to America? You mentioned Bennett putting his feet on the table and people using first names.

LS: Yes. Of course, you know this old building, this was quite different from our institutes.

VM: How was it different from your institutes?

LS: In Cologne we had a completely new institute, with modern laboratories, and this was like a barrack or temporary building. It was because the second time I came to Berkeley they were in the Life Science Building because a new building was erected at the time.

VM: But did you find it an unpleasant building to work in?

LS: Not at all because the people were all very engaged in their work, very busy and helpful, and the atmosphere was very exciting. It was even more active because it was, of course, a research laboratory and in my institute there was the teaching of students. This concentration to the research was a wonderful time for me.

VM: Apart from the building, did you find the American style of working, as you saw it at that time, different from the German style that you had come from?

LS: We had very good facilities and I had the impression that they had lots of money and especially with this radioactive labelling. They had lots of experience. I think that in their methods they were ahead of us. This was the reason why they asked me from the (*German*) government to learn over there.

LS: When you had been in Germany before, and perhaps also when you came back, what was the relationship between the professors and the other people? Was it more formal than in the American situation?

LS: Yes it was, more formal. Not today any more, but at that time it was more formal. The students used to say "Herr Professor", "Frau Professor". Today they don't do it any more. When they go out of my room they say "Tschüss". I don't like this; I think Tschüss, this is so...

VM: Do they now call you by your first name?

LS: No, they don't do that.

VM: They still give you a title?

LS: Usually not a title: "Frau Stange".

VM: Oh, I see: it's that sort of title. But not "Luise".

LS: No, they don't do, no. You see, in Germany we have this difference between "Du" and "Sie" and you don't have this in English language, so it already makes a difference.

VM: The German students use "Sie" and are more formal.

LS: There are now younger professors who ask that they shall say "Du", but I don't like this specially because I think there is a difference between a teacher and a student. It's not a value, but a difference in the function.

VM: So, anyhow: America in 1957 — by the way you came in '57 didn't you?

LS: In '57.

VM: For a year, you said...

LS: For a year.

VM: ...the first time.

VM: At that period it was less formal than Germany at the same time, in your experience: in '57.

LS: Yes, it was.

VM: How about the social life in the lab? You say you worked day and night, but I am sure you relaxed as well with the other people. What did you do?

LS: It was a wonderful time, you know, this country, with the mountains, high mountains...in winter we went skiing, and I enjoyed this very much. We even had a trip to Sequoia National Park and we slept there on the ice. It was wonderful; we crossed a creek and I lost a shoe in the creek and Ozzie Holm-Hansen gave me one of his spare shoes. And there is a picture even of different shoes on my feet. It was a wonderful trip. Then, of course, in summer we also had wonderful trips. Of course, I remember the trip together with Vivian and Sheila Moses and Erminio Lombardi to all the national parks in the Southwest. It was a wonderful time.

VM: It was a wonderful time for all of us. You acted very freely socially with people in the lab.?

LS: Yes. It was a first Christmas away from home and I was invited at the Holy Evening to Karl Lombardi's house. I remember this quite well...they had a goose...

VM: to Karl Lombardi?

LS: No, no, no: Karl Lonberg...and he had a goose, a fried goose...and then we sat down and listened to Handel's Messiah. It was wonderful because I like this music very much. The next morning, the first Christmas day, I was invited to Martha Kirk's house out there in Walnut Creek; I think it was Walnut creek, some place there. Her parents were still alive and I got some presents under the Christmas tree and everybody was so nice. A wonderful dinner; I think it was a dinner. They were all so...*was heisst* "*Gastfreundlich*"? Hospitable, very hospitable. They always took me with their car, I didn't have a car then. Even I did only my driver's license over there at the end of the year, so they had to take me in their car.

VM: So you never had a car at any time in California?

LS: No, in California not.

VM: How well did you speak English when you first arrived?

LS: Oh, you see, I don't think it was too good but it was good that I was alone so I was forced to speak always English. There had been German couples and they always spoke German when they were in their house, in their living apartment. I was forced to speak only English and I think I improved very much at the time. I remember when I came home to Cologne I even used English constructions because in English it is much easier than in German because of the use of participles which shortens the whole sentence. German is much more complicated in the construction.

VM: So you learned to do this in the year in California?

LS: Yes, yes, yes, I did.

VM: Was it tiring to have to speak English all the time?

LS: Not at all.

VM: So you must have spoken quite well when you came in order to be able to build and feel fairly comfortable.

LS: Of course, I learned English in school but I had never been in an English-speaking country before. This was my first experience in an English-speaking country.

VM: When you went back to Germany at the end of...when was it? In the summer of '58?

LS: In the fall.

VM: Were there parties when you left?

LS: Yes, there was a goodbye party.

VM: Where was your goodbye party, do you remember?

LS: Somewhere outside and we had to go there. There were some eucalyptus trees and they made a barbecue; I have even pictures of this.

VM: A lab. party, people from the lab. came?

- LS: Yes, from the lab. Dr. Calvin was also there. He joined the goodbye party. They gave a card to me with all the names on it; it was like a gun with a big *Kugel*: *was ist das?*
- VM: Bullet.
- LS: Bullet to shoot me back because I stayed there 'til the last minute. I think I still keep this card somewhere! Could be: all the names were on it — very nice, yes.
- VM: What did you think of Calvin as the leader of the group? Did you find him an interesting person, exciting person? How did you deal with him?
- LS: It was always interesting. I met him mostly in the seminar. This was very stimulating because he had always new ideas and important questions. Also when I gave the seminar it was very good for me that he had the questions which partly I couldn't answer. I had to look afterward for the answer. Then when we had to go thorough the manuscripts, this was important for me too because at that time I learned that in every publication you have pictures or graphs and it is necessary to have under the graph a complete description of what is shown. Dr. Calvin said to me "Luise, you have to write a paper. Nobody has time; you have a paper like — you have only look at the pictures that you know what has been done." This I followed in all my further publications. It was very important counsel.
- VM: Had you published any papers at all before you went to Berkeley?
- LS: Oh yes, I did.
- VM: before you went to Berkeley?
- LS: Yes, but only in German.
- VM: When you wrote papers from that lab. there, did you write them yourself? You could write English well enough to make a first draft?
- LS: I think I got quite a bit of help from Dr. Bennett and Dr. Moses in writing these papers; yes. Especially the fractionation paper, the fractionation of the insoluble material; I remember that Dr. Moses was sitting in front of his machine and he started with the introduction and the introduction always is to cite other papers.
- VM: But I noticed that, at least on many of these papers, perhaps all of them, you are the first author.
- LS: Yes, because I had done most of the work which is included in these papers.
- VM: If you were the first author, then probably you did most of the work and you wrote the first draft? That was usually the style.
- LS: But you helped me, especially with the fractionation paper and Dr. Bennett. Maybe the last one I have written mostly myself but Dr. Calvin went over it and asked something to improve it.
- VM: You left Berkeley in the fall of '58 and came back to Germany. What did you do when you came back?
- LS: I went back to Cologne...

VM: ...to the same lab. that you had been before?

LS: Yes. Because before I went to Berkeley in February '57, did my *Habilitation* (what is usual at German universities) so I was then a *Dozent* in that laboratory. I stayed there until '63 or '64, I don't know; don't remember exactly.

VM: But you went back to Berkeley for a period?

LS: Yes, in '60.. Actually, I wanted to go back to Berkeley earlier but they were moving out of the Old Radiation Laboratory into the Life Sciences Building. My father was very old at that time and actually he died in December '59. That was the reason I postponed my second visit to Berkeley. Finally in '60 I could come a second time.

VM: Why did you want to go back? I know that it's a nice place and so on; but specifically?

LS: No: Dr. Calvin actually wrote a letter of recommendation so I could get money again. And he wrote that I couldn't reap the fruit of the trees which I had planted. Actually, from my first visit there had been the two papers, fractionation and short-time incorporation. And then incorporation in the insoluble material: this is from the second stay, from the second visit. So I had prepared the method, I had worked out the synchronous culture but I had not yet labelled the nucleic acids during the synchronous growth to the cell cycle.

VM: When you went back a second time, the lab....the people were then in Life Sciences Building?

LS: Yes, They were in Life Science Building and I had to use the steady-state machine of Dr. Bassham. I got quite a little bit of help from Martha Kirk at that time.

VM: How long did you stay that second visit?

LS: About four months at that time.

VM: You didn't got back to your old apartment, did you, on Hearst Avenue?

LS: Yes, I did.

VM: To the same building?

LS: To the same building, not the same apartment, in the same building, to Mrs. Lee.

VM: How did you notice the change in the group which had moved from ORL to the Life Sciences Building? Was the atmosphere different at that time?

LS: I don't think — there had been some new people...one from Britain, I don't remember his name exactly now...a couple of Britons, some new people have been there, some foreigners, guests in the laboratory, but the American staff was the same.

VM: Were you able in that second period to complete the work that you had wanted to do? Was there enough time?

- LS: There was enough time although I took with me some chromatograms to measure them in Cologne to finish it, but it was enough so that we could publish this last paper from this period about the incorporation of $C^{14}O_2$ in nucleic acids during the cell cycle.
- VM: That was the last time you worked in Berkeley?
- LS: This was the last time you worked in Berkeley.
- VM: But you went back from another visit later. When was that?
- LS: I think it was in '78.
- VM: When the new building, the round building.
- LS: It was in the new building. I joined a seminar and Dr. Calvin was very nice. He told the people, most of them I didn't know, that I had been working there before twice and it was very nice to be in this group again which was guided by him.
- VM: You had worked in ORL and then you had worked in Life Sciences Building and then in '78 you saw the round building.
- LS: Yes.
- VM: What did you think of it?
- LS: I think it's a marvellous building, probably...you see, I didn't work there...
- VM: I understand.
- LS: I had the impression that they had very good working conditions in this new building now.
- VM: You know that that building was an attempt to recreate the atmosphere of ORL in a modern building. Do you think it was successful?
- LS: No, it was completely different, I think. As I said before, this was a special atmosphere in this Old Radiation Laboratory. We had lots of fun. It was very nice. But it was a special atmosphere because of this old building but this active group, this combination; it something special.
- VM: It was not possible to reproduce that at a later stage?
- LS: No, I don't think so, no, no.
- VM: OK. Let's come back to 1960, November 1960 — October/November — when you went back to Germany again. And again, back to Cologne?
- LS: Again, back to Cologne, yes.
- VM: You stayed, I think you said, until '63 or '64.
- LS: '63/'64. And then I was offered a professorship, I was already a professor in Cologne but it was called *Ausserplanprofessor*; it's only the title. Actually, you have to publish

some work. Of course, I published something else beside these papers. Then, two professors have to be asked if a person who has his *Habilitation*, taught for some time, is able to have a chair. If there is no chair, they get the title *Ausserplanmäßiger-professor*. One of the professors who was asked to recommend it was Dr. Calvin and he wrote, apparently — I have never read it — but my professor said it was a very good recommendation and the one was Folke Skoog in Madison, Wisconsin. Apparently that was also...He was interested always in my work about regeneration.

VM: Did you know him personally?

LS: Yes, very well. He visited us several times in Cologne and Hannover and we had many interests in common.

VM: You know, of course, that Ozzie was a student of his?

LS: Yes, I know, I know.

VM: When you left Cologne, where did you go?

LS: To Hannover. At that time it was called the Technical University; it's now University. I worked there in the Botanical Institute but I had the Division of Plant Physiology.

VM: That was your title, Professor of Plant Physiology?

LS: Yes. At the same time, I was teaching at the medical...it's called medical high school (*Hochschule*; it is different from the English high school — a medical university, you might say). I enjoyed this work very much with the medical students. It was general biology and we had them also in our practical courses for medical students.

VM: And then you stayed there how long?

LS: Nine years...

VM: Until early '70s.

LS: ...until '73. In '72 I was offered a chair in Kassel at the new university and it took a whole year to...because I had this good position in Hannover at the Medical School which also paid my teaching so I took some time until I did get good conditions in Kassel. And then I went on there and I had my own group there, in a new institute. It was only a rough building and I could put in everything I needed. It was very nice and I had my own new group there.

VM: That, of course, is just what Calvin did with the Old Radiation Lab. He always had a building in which he could put his own stuff and build it the way he wanted. Were you influenced in what you did in Kassel by what you had seen in Berkeley?

LS: Yes, a little bit. Because then I got offered a share: I said I need to have a radioisotope laboratory. I made the plans for it; it's now too small and just now they build a new one. I have been there for 20 years in Kassel. For this time it was our...for the whole faculty...also for the chemists it was our radiation (?) laboratory, radioisotope laboratory.

VM: How about discussions and seminars when you had your own group? Did you run a seminar the way Calvin used to run a seminar?

- LS: Yes I did, with my own group, yes, I did.
- VM: You would ask questions all the time?
- LS: Yes, I did too.
- VM: So it seems that you have been quite influenced later on by the time you spent in...
- LS: I must say that also in Cologne, before I went to Berkeley, our institute had a seminar when I was still an assistant at that time.
- VM: Looking back on your time in Berkeley, what do you think it did for your career?
- LS: Two things, I would say. First of all I learned many new methods in laboratory work, small things which are very important and apparatus; this is more methodological. The other was even theoretically, as I said, because at that time I was only interested in the activation of the cell cycle, but now also in the normal cell cycle where cells divide all the time, I now see the differentiation very close to the cell cycle. Even in my later paper I have some hypothesis that differentiation comes about by differences in cell cycle, at special stages of the cell cycle, which are, of course, metabolically characterised, have influence on neighbour cells and can induce directed transport. Because in differentiation directed transport is a very important phenomenon. So now the cell cycle is really in the centre of my work although I'm interested in cell differentiation. I think it was very important for my theoretical position in research too, my science too.
- VM: That was the first thing. You said there were two things that you learned.
- LS: No, the methods was first, and this is a theoretical point of view.
- VM: What sort of contact have you kept with people from there? I know that we see you from time to time — not very often, but every few years. What about contacts with people generally from your period in Berkeley?
- LS: I had very, very close contact to Martha Kirk. She has been my very, very good friend. She worked for some time in Germany with Ulrich Heber and then with Karl Erismann in Bern. From there she came very often to see us at the weekends in Göttingen. She has been here very, very often. I had close contact with her. Then, of course, to Ed Bennett. He visited me several times, too. We had wonderful trips in Berkeley when I was there in '78 we had wonderful trips again and I am still in contact with him by letters. Otherwise, I have no contact any more. Of course, I had also besides the laboratory I have met other Americans and to them I have very close contact too, completely out of science — a whole story by itself; very interesting people.
- VM: It seems that you had a good time and that you retained good memories of the periods in Berkeley and it has been helpful in your career.
- LS: It certainly was very helpful. I have the best memories and I am glad that you are here so I can make them up again!
- VM: We are very grateful that you have contributed to our collection of interviews and it was not as terrible, I'm sure, as you thought it might be. It was no inquisition!

- LS: You see, I tried yesterday evening, but it was too late to read these papers so I could get, give you...but you could read them yourself.
- VM: I can indeed.
- SM: Did you get to know the Calvin family at all?
- LS: Yes I did. I was invited several times to his house and enjoyed this very much. Especially his wife was very, very friendly. She was a very wonderful woman, I think. I have even seen his daughters and the little Noel. I was very much impressed one evening. He was watching television and Dr. Calvin said "now you have to go to bed" and he didn't want and he brought him back but pretty soon he was back again to television! I told people in Germany that children even govern sometimes their very important parents. Mrs. Calvin gave me as a goodbye present an etching perhaps of the tower, of the campanile in Berkeley, and I have it hanging in my working room in Kassel. It was very nice. They are good memories.
- VM: I think it remains only to say thank you for contributing and this tape will now be preserved for posterity!
- LS: Thank you very much for coming and to honour me with this small work I have done in Berkeley. Thanks you so much.

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BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name LUISE, M.C. STANGE
Date of birth MAY 6, 1926 Birthplace GOETTINGEN
Father's full name CARL STANGE
Occupation UNIV. PROFESSOR Birthplace HAMBURG
Mother's full name MARIE STANGE, NÉE DUNKEL
Occupation TEACHER Birthplace HAMBURG
Your spouse /
Occupation UNIV. PRO Birthplace _____
Your children /

Where did you grow up? GOETTINGEN
Present community GOETTINGEN
Education DR. RER. NAT, UNIV. FREIBURG I. BRG.
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Areas of expertise DEVELOPMENTAL BIOLOGY

Other interests or activities _____

Organizations in which you are active _____

Chapter 51

PETRONELLA (NEL) PRINS VAN DER MEULEN

Delft

May 29th, 1997

VM = Vivian Moses; NP = Petronella (Nel) Prins van der Meulen; SM = Sheila Moses

VM: This is a conversation with Nel-Prins van der Meulen in Delft on the 29th of May, 1997.

Can I start by asking you what your early education in science was and, therefore, how you came to be in Calvin's lab.?

NP: I did study at the University of Leiden, I took chemistry with Professor (*Egbert*) Havinga (*Editor: Havinga had been a visitor to the Bio-Organic Group at an earlier period.*) As organic chemistry was my main subject and biochemistry was my second subject — it wasn't possible to study biochemistry as the main subject at that time, which would have been my preference — then Professor Havinga had connections with Professor Calvin, they knew each other. He sort of made it possible for me to come into contact with Professor Calvin. Havinga made it possible for me to work with Professor Calvin. I had a grant from the Netherlands Medical Foundation and a travel grant. I have been able to study for a year at the Berkeley lab. I had finished my studies, which we call "doctoral exam" in Holland, but I think it's comparable with a master's degree in the United States. So it was right after my study that I came over.

VM: Had you made any previous arrangement with Calvin about what topic you would work on

NP: No, we didn't. I just arrived and he sort of told me that it might be interesting for me to take that subject of finding the radicals that would be involved in photosynthesis. I started on that part but there was no equipment to work with; there had to be a lot of work done on it. My physics background wasn't very strong; that was partly because of the war where I had missed the first parts of physics and sort of picked it up in university, which is not a very solid base. After, I think, about two months or so I quit that part: there had to be too much work on the equipment and not the research itself.

So there was another subject which I had been working on later on and that was much more in my line and I enjoyed doing that.

VM: Which was what?

NP: That was investigating the influence of some inhibitors, enzyme inhibitors, using the building in of $C^{14}O_2$ into algae and then seeing when you use the inhibitor what was the change in the labelling of the compounds. By sort of shortening those experiments

you could find out where the inhibitors would attack, which enzymes would be inhibited and so you could find the place where they were working. That was mainly what I have been doing using the working methods of Calvin with algae and $C^{14}O_2$ and then chromatography, finding out what was the difference between the compounds being labelled in the normal set-up and when you put in the two inhibitors.

VM: What were you looking for?

NP: Just to find which enzymes were stopped by these inhibitors.

VM: Did you have any particular reason to suspect which enzymes might be stopped?

NP: Not so much in the beginning, I think.

VM: So it was very much a “shot in the dark” to see what would happen.

NP: To see what was the change and then, by reasoning and then changing conditions, find out where the inhibitors would work.

VM: How did you learn everything? Did you work with someone when you first got there to learn all the techniques that were in use at the time?

NP: When I started on this subject, I sort of learned the techniques of chromatography which were used in the lab. I think mostly it has been Al Bassham that introduced me into that. Then I started...well, my own variation of the techniques that had been used before.

VM: I think you said you arrived there early in 1956 and you stayed about a year?

NP: Yes.

VM: I'm interested in what happened. How did you travel when you went to America?

NP: By boat, which was the cheapest way at that time.

VM: And across the country?

NP: By train across the country.

VM: When you got there, was there an apartment waiting for you or did you have to find one? What happened?

NP: I stayed at International House; this was arranged beforehand. Calvin wasn't around at that time so I had been about a week or so, I don't exactly know, but I have been about a week in International House before he arrived in the lab. When I arrived in Berkeley, I was a bit sick so I needed that week anyway to get well again.

VM: So, you actually started working after Calvin arrived and you were back in the lab. and met the people there?

NP: Yes.

- VM:** If I can ask you just a couple of questions about the first piece of work that you did: you said this was something in which you really had no experience, working in free radicals in photosynthesis.
- NP:** No, I had no experience.
- VM:** How come that you and he agreed that you should do this with no experience?
- NP:** Well, he thought it would be nice...in a way it was a nice suggestion and it could have worked out maybe well if the equipment had been around. But they were still building the equipment by themselves, there was not a fixed apparatus like you had a few years later. He sort of built his own equipment and if it didn't work quite as it should... So there were difficulties in the equipment part which was technical and physical and this was not something I could cope with very well.
- VM:** Who was doing the building?
- NP:** I think it was Power Sogo.
- VM:** I don't remember; this was not something I worked in. And he did that by himself?
- NP:** Yes, he sort of built it himself. It wasn't an apparatus that came from a factory or so. He did it himself. It was in the very beginning time of this kind of investigation.
- VM:** When you settled down to work in the carbon-14 part, you worked, presumably, in ORL, in the old wooden building. Do you remember who you worked with, where you were?
- NP:** Well, I think...you know how it looks, the lab. You came in, then you had a sort of a small office part where Al Bassham had a bureau or something like that, and then you went through a door and came into one of the rooms and I had on one of those tables I had my two metres of table and the part of the wall that belonged to it and the things down there that belonged to it. There you had to do all your work except for the chromatography you could use the special room.
- VM:** Do you remember who was next to you?
- NP:** No, I don't.
- VM:** What do you remember of the atmosphere of that building at the time?
- NP:** There are a few things which were very nice. The first thing, that was when I arrived and started to work there, I was given a key so I could get in and out of the building every moment of the day I wished to, which was something for somebody who had just finished his student times, it was something very special. I enjoyed that. I worked quite often because I had to take out chromatograms on all parts of the day. I was there sometimes at night. There would be night watchers around with their lanterns and it was a very special atmosphere. Then the Geiger counters that were rattling around, normally very quietly, but if they brought in some phosphorus they would rattle away quite happily. Those were some special atmospheres but on the whole the people were very nice with each other and very helpful. When I had problems there would always be somebody to help me out, to know what to do, to find where things were and so on or to find the right person to ask. One of the things which was a very positive side of the whole atmosphere of the group was there were so many foreigners

around that they organised trips every now and then and this got some very special atmosphere, too.

One of the things which I remember very well also is that every now and then there would be — quite often, I think, at least once a week — Professor Calvin, Dr. Calvin, would come in and he would ask what everybody had done, look at the chromatograms if you had some new ones and then compare, he would see what was special about this, talk about it with you. This was very nice. He was always interested in your progress and would give some advice and would talk it over. And in this you would do with everybody else around, which was very nice too. That was informal and a very nice part of working there.

VM: Was this different from the life that you had led in Leiden before you went there?

NP: Well, yes. But also because I had been a student and hadn't been working on myself. I had been assisting in the practical work of the younger students but I had not done any research in the lab. there except my own special subject for finishing the studies. There you had a connection with two or three other people that worked in the same fields, and Professor Havinga then, but not this whole group because he (*Havinga*) had very different subjects and so we also worked in different rooms. Here it was all crowded into one very small and, in a way, very unhealthy laboratory. If I now realised that we were doing chromatography in the room where everything was saturated with the vapours which you should stay away from, I'm still happy that everybody has been healthy for such a long time. Also the other workers, apparently, because they are around! I think I have had a lot of benzene taken in, inhaled at that time...

VM: And probably C¹⁴ as well.

NP: C¹⁴ as well, I guess, but you didn't smell that; benzene you did!

VM: I certainly agree that the building was crowded and old and...

NP: But that also gave it a very special atmosphere. You had to do it together with what there was and you had to, well, to give some room to your neighbour and so on.

VM: You think that part of the whole atmosphere in the group and the way the group developed was really influenced by the building in which they worked?

NP: I guess in a way, yes. It had some sort of a pioneering atmosphere. Not everything was there. You had to make the best of it. This, of course, people who can't stand that will leave. I think it was, in a way, positive but only because, I guess, the work was interesting that people were doing there. If the work hadn't been interesting and you had those circumstances, you wouldn't stay. The work was interesting and take all that because everyone saw it was interesting you got this special atmosphere.

VM: Did you find that Calvin was very good at new ideas? You say that he was interested in what you did. Was he perceptive, was he quick to see the significance of the results that you gave him?

NP: Yes.

VM: Did he steer your work or did you really decide yourself what you were going to do and then tell him?

- NP:** No, it was a sort of exchange, I wouldn't quite remember. I didn't quite feel that I was only doing what he said and I didn't feel hampered that he wouldn't like me to go on. I think it was just...well, seeing him quite frequently and talking about the work that things went along that line. I also did talk with Al Bassham because the work was quite near to his experience and his subject.
- VM:** So really Calvin and Bassham, the two of them, were the people that you most interacted with.
- NP:** Yes, I most interacted with and with the others every now and then, but not on my own regular work.
- VM:** Presumably you gave seminars, or at least one seminar, in that Friday morning series they had?
- NP:** I guess so, but I don't remember.
- VM:** You don't remember that Friday morning business?
- NP:** Yes, well, I remember, but I don't know whether...I must have been talking there.
- VM:** You left there at the end of '57...'56?
- NP:** The end of '56, beginning '57 somewhere around Christmas time, January: somewhere around there.
- VM:** You were present at the Christmas party, I think; you showed me some photos of the '56 Christmas party. Did you have a farewell party as well, when you left?
- NP:** I guess so.
- VM:** Too long ago?
- NP:** Too long ago.
- VM:** Then you came back to Holland?
- NP:** I left in a way that I experienced as pleasant and I had a dinner with Mr. and Mrs. Calvin and some other people which was near the end, near the time I was leaving.
- VM:** You were yourself not married at the time?
- NP:** No, I wasn't married.
- VM:** Then you came back to Holland and resumed your scientific career here at the time.
- NP:** Yes, I started to work with TNO then...
- SM:** an you say what TNO is.
- NP:** TNO is an Organisation for Applied Scientific Research.
- VM:** Here in Delft?

NP: Here in Delft.

VM: Is it part of the university?

NP: It is not part of the university, it's a separate institute.

VM: You were there for some years?

NP: I have been working there for, I guess, about five years.

VM: What happened then?

NP: In the meantime, I married (in '58) and in '59 I got my son and in '61 I got my daughter and in '63 I was expecting the third one. And then I couldn't quite keep up with the research. Also, when I was expecting the third child the young girl that had been helping me in the home and taking care of the children while I was away was going to marry herself and wouldn't go on working with me. I could not find somebody that I could trust three children with, at that time. So, I decided I had to break and care for my children for a while at least. I always thought I will go back into research when the children are in school.

So, when the youngest was four years and would go to kindergarten, I looked around for a job and asked my professors, especially the one in biochemistry. He made it possible for me to find a job in Leiden (I think it was in *indecipherable*) — it was quite a nice (*indecipherable*) — and everything was almost organised and I would start work about one and a half months from then. I had the literature and, well, trying to read myself into the subject because it was a new subject and I had to do quite a lot of reading. You know in biochemistry things develop so very quickly so all the methods that I knew I had to jump into that.

I found that I couldn't combine having enough attention for the children with this research work. It was so interesting, it so takes your mind, that I couldn't be open for the children at the same time. I found myself saying when my son was saying "Momma this" or "Momma that", "Wait a moment, Momma's working" and so on. I had all those little moments that I saw that this is not going to work, I can't do that. If I am not there for my children at that moment, they will not come to me when they need me in later trouble. So this was a very crucial part of my life and I had to decide. I had always thought that at least I couldn't do it, I couldn't divide myself between the things. So it had been very difficult. My husband had quite a heavy job, he couldn't work less or anything like that. He became soon after director of the Hydraulic Research Laboratory in Delft. He's older than I am so his career came before mine.

I decided at that time that I had to choose between children and research and so I decided it had to be the children. I went to my professor and said that I could not take the job. He was taken aback, he was mad at me. He said "why didn't you know before?" I said that I didn't know before and I explained to him what made it clear to me that I couldn't cope with these things together.

So that was the end of the research. I knew that if I didn't jump into it at that time, which was four and a half years after I stopped, well things develop so quickly, that I wouldn't be able to take it up later on very likely. Then I decided, after half a year, that I wanted to do something out of the house, something that would not occupy

myself that much. So I started to take a few hours at secondary school teaching chemistry, just to see whether I liked that or not. It turned out that I had no experience in it before but I could do it and I quite liked and I could very easily work part time and would be free when the children came home from school and would have holidays when they had holidays and all the problems were away. What was most important for me, I could teach and, of course, you have to do some work next to that but I could just do that any time — it didn't occupy my mind. It was something that could be done sometime but it wasn't something that would go around in my head — that's what research did, it went around in my head and I had to think about how to cope with this, how to do this and that. With teaching, I had no problems; I could easily separate home and the teaching job. I have been teaching for twenty years in secondary schools.

VM: The research that you did do, I guess when you did your master's work here and in Berkeley and perhaps later, were all exciting times of your life?

NP: Yes, I liked that very much.

VM: Have you been back to Berkeley since those days?

NP: Only...I have to think when it was; I think it was '92.

VM: That was the first time?

NP: The first time; I have been back in the United States but not in Berkeley before.

VM: Did you see the round building?

NP: The new building? Yes, I had a short look into it. I had never worked there so it didn't say anything to me. I had no emotional connections with that.

VM: And of the people whom you knew...

NP: There was only Ning Pon around and nobody else.

VM: Nobody else at all?

NP: No.

VM: That must have been rather disappointing.

NP: That was a pity, yes. But I was sort of prepared that many of the people weren't around any more.

VM: So it seems that you have a fond memory of those times at any rate.

NP: Yes; I have a very fond memory. It is long ago. It has been a very nice year of my life and very interesting. Well, I worked after that in research for five years and that was the end of research. In a way that's a pity but on the other hand...well, sometimes you have to make choices.

VM: Children have their compensations. It was very kind of you to take time. It was nice to have seen you again after all that period and to find my photograph in your book, even though you weren't quite sure who I was.

NP: I will write it down now.

VM: OK; thanks a lot.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name van der Meulen, Petronella Yke Fokje

Date of birth 10-07-1930 Birthplace Appingedam (Nederland)

Father's full name Jelmer van der Meulen

Occupation deceased Birthplace Sneek (Nederland)

Mother's full name Hendrika Hanwer

Occupation deceased Birthplace Hongereen (Nederland)

Your spouse Prins, Jacob Egbert

Occupation former director Hydraulics Birthplace Culemborg
laboratory

Your children _____

Robert (son) Yke (daughter) Hajo (son)

Where did you grow up? Meppel, Haarlem (Nederland)

Present community Delft (Nederland)

Education university of Leiden - chemistry/biochemistry (1948-1955)

Research fellow at the University of California, Berkeley 1956-1957

Occupation(s) 1957-1963 research at TNO (Applied Scientific Research
organisation)

1968-1988 teacher of chemistry at VWO (pre university level
16-18 years old pupils) both in
Delft,
Nederland.

Areas of expertise _____

Other interests or activities _____

Organizations in which you are active Humanist Association

Chapter 52

JACQUES MAYAUDON

Louvain-la-Neuve

May 31st, 1997

VM = Vivian Moses; JM = Jacques Mayaudon; SM = Sheila Moses

(Editorial note: Dr. Mayaudon's spoken English is not good. He used a number of French words and both these, and some of his remarks in English, are often difficult or impossible to decipher. The discussion was attended by colleagues who intermittently offered translations of French words and phrases; some of these can be heard on the recording but have not been transcribed.)

VM: This conversation is with Jacques Mayaudon on the 31st of May, 1997 in Louvain-la-Neuve and he's just put on the table a book on *Isotopic Carbon* by Calvin and his co-workers.

JM: Thank you, my colleague. This book has changed all my life, professional life. I had this book in 1950; I understand what I must do for my research and I spoke about this book to my professor, *directeur*, Professor Simonard and also the Director of IPSEA, Institute of Research, which paid me. All these persons understand why I should want to go to Melvin Calvin's. For this reason I learn English and I take the *Elizabeth* boat transatlantic to New York...

VM: Oh, the *Queen Elizabeth*?

JM: The *Queen Elizabeth*, yes. I stay one week in Tudor Hotel and after that I took the airplane past Chicago and I go to Berkeley. After I look after a (*pace to*) in Berkeley, my first visit is for Professor Calvin.

VM: OK. Can I ask you some questions about your education when you were young?

JM: I was always interested by understanding the action of the environment of health, the chemical and microbiological process; the physical process also.

VM: When you were a student, which university did you go to?

JM: Always in Louvain.

VM: And what did you study when you were...which subject did you study?

JM: I was an engineer chemist in the agricultural industry.

VM: An engineer in the chemical and agricultural industry. When did you finish your studies as a student?

JM: I studt, I finished in — wait a minute now...I don't say the mistake — in 1948 and immediately at this time I wanted to go to the chemistry laboratory in the State and we had always an examination and people, as you, take a question: it was Professor Simonard. He looked (*at*) me and immediately he said, please come to see me. So to finish: at this time I study about the problems about cheese, Belgium cheese like (*indecipherable*), and this was the head of my doctorate — understanding the biochemical process of maturation of this cheese. And this, it is very important for me, because I isolated the microbe which was not *Bacterium mimens* as people said but a new one — a microbe which produced indoleacetic acid. It is a growth factor — indoleacetic acid.

VM: And so you finished in '48...

JM: I finished in '54.

VM: Your thesis?

JM: My thesis. And a week later, because I received a fellowship from IPSEA, I go and see Professor Calvin so all is clear.

VM: Right, now all is clear. So you arrived in San Francisco or Oakland by aeroplane.

JM: All people know IPSEA and Professor Calvin. This is interesting: we must go and see Professor Calvin because in Belgium we must apply isttopic carbon.

VM: So you had already written to Calvin to explain your interest.

JM: Yes, sure.

VM: And had you agreed with him what you would do in Berkeley?

JM: At Berkeley it is not the same because all people were studying about the photosynthesis process with algae. But at this time it was difficult to collect and purify the enzyme. It was not possible, not possible. So for me it is algae (*indecipherable*); like Shibata said, it is not good for me. I want to see a vegetable or algae which it could be extract enzyme. So I go and see about Berkeley mostly about Sunday or Saturday because all is free. I look and I chose the best one using spinach, not to eat but to extract the enzyme.

VM: But when you arrived in Berkeley, where did you live?

JM: I live with a very kind person, a pensioner, on a small street or avenue, very kind, very kind. I eat at the Faculty Club. I was a member of the Faculty Club.

VM: How did you find the place to live?

JM: By myself. I look. I see to rent...

VM: I see. You had no car?

JM: No, never. Because car cost money. I had a six-month grant; I stay one year.

VM: So you were always short of money.

JM: The problem, I can show to you the papers, it was impossible to take...even in summer time you must work in the lab., day and night.

VM: In order to produce results.

JM: Before I go to Calvin, I go to the Donner Laboratory. Here I speak, to look hard to do label carbon-14 labelled like a vegetable, like cellulose. In Donner laboratory we can do. When I come back here I prepare by myself labelled cellulose, labelled enzyme, when I say labelled, it is carbon dioxide it is carbon-15, no?

VM: 14.

JM: *Oui*, carbon-14.

VM: Who did you talk to in Donner Laboratory? Who was the person that you talked to?

JM: It was a very kind woman, her name now I forget, but her superior was Tolbert.

VM: I see, yes.

JM: Very modern, very modern.

VM: So you knew Tolbert well.

JM: And changed all things this for all my life: why? Because I was interested (*indécipherable*) because he looked at rats and mice. "I don't know," he said, "metabolism." This is when (*indécipherable*) because when I will back to Belgium I will change mice rats by microorganism, by soy. For this reason I write a book I can show to you. *Application of Radiorespirometry Designed by Carbonation*. Because I went to this Donner Laboratory. If I don't go...if I go immediately to photosynthesis, this was not possible...I have no idea.

VM: How long did you spend in the Donner Laboratory?

JM: Six months.

VM: Six months!

JM: And six months with Benson. You can see the papers — day and night, day and night. (*Indécipherable*) People said "will you burn your candle both the both sides?" When my mother say to me, "Jacques, *allez*, go and eat now, you can sleep now and you can stay now".

VM: So you had no social life at all?

JM: No.

VM: Social life: you did not have friends there?

- JM: I had friends — Bourdon. But in the lab. my best friend was Benson, Andy Benson, because he understand my objective. He was also very interested by extraction of enzymes. We both, we give the name of the enzyme...
- VM: Which name?
- JM: Ribulose carboxylase.
- VM: Oh, you and Benson gave the name...
- JM: We have the only, both, we study because we can extract the enzyme.
- VM: So when you had been in Donner for six months you moved to the Old Radiation Lab. to work with photosynthesis, yes? And by that time you already knew how to use isotopic carbon...
- JM: Oui.
- VM: ...which you had learned? And you talked to Benson about the enzyme?
- JM: About the enzyme.
- VM: And what did you do?
- JM: And, with his help, he chose for me the laboratory when I could go on. You can see the picture, the process. It was not from Melvin Calvin — it was not possible. He has not the the apparatus.
- VM: So did Calvin know about what you were going to do? Did you talk to Calvin?
- JM: Yes, but you see from this, for instance, not from Calvin's lab. But *alors*, one thing, in '55 or, I think, '56 Andy go away, I think as a professor in Pennsylvania University.
- VM: That's right, yes.
- JM: And I tried to have a contact but it was impossible to have a contact with Benson and Melvin was very occupied. I saw him in Brussels. He was very kind with me, Benson, because I make chromatograms like this one (*showing some papers*). (*Indecipherable*)
- VM: Where was this paper published?
- JM: At Brussels.
- VM: In the International Congress of Biochemistry? '55, yes.
- JM: And Benson, was, I remember, at the Metropole Hotel.
- VM: In Brussels.
- JM: Yes. So I invited him, his chamber...
- VM: ...room, yes.

JM: Room; it is...

VM: Suite?

JM: Large apartment. His wife was there, I think, two boys, two people, girls I think. We had a very good meeting with him. But you know the problem is, after this any silence.

VM: But when you met Calvin — you met Calvin in Brussels?

JM: Yes, yes.

VM: This was '55, before you went to Berkeley?

JM: No, I went already. I come back in January '55.

VM: I see. And you were in Berkeley for one year?

JM: *Oui.*: January '54 to '55.

VM: So you saw Calvin...?

JM: And six months later I saw...

VM: I see. And then its...

JM: And here is the (*indecipherable*) published three chromatograms from my experiments.

VM: I understand now. OK.

JM: I saw to him and we will publish together and these papers were here....

VM: So this is a paper with you and Benson and Calvin in 1957, *Biochem. Biophys. Acta*.

JM: It was accepted and it was published in *Biochemica et Biophysica Acta*.

VM: How far did you succeed in extracting the enzyme? Were you able to purify the enzyme?

JM: Yes, yes, yes. It was purified.

VM: It would fix carbon dioxide in a test tube?

JM: All things..

VM: So, you were the one, really, the first one to work with the actual enzyme that fixes carbon dioxide?

JM: (*Indecipherable*)

VM: Right. I see. That was very interesting, then. So that was a very important contribution that you made in Berkeley in '54-'55. And you say you had to work very hard in that period to do this work.

JM: Very hard.

VM: Did you have an opportunity to visit other places in California or in America?

JM: I know also on the campus a person...(Indecipherable)...I went and I appreciate the conversation with Stanier. Stanier was interested in *Agrobacter*. And Doudoroff also. This laboratory was interested by application of carbon-15 (*sic!*) to bacteria metabolism. In the evening I was and during the day I worked with Calvin. And in the night I work...

VM: You worked with Stanier and Doudoroff. Did you publish with them as well?

JM: No. By their help, I finally purified the bacteria from cheese and this bacteria was not yet published but I am sure that later on (even if I will [*have*] died) this bacteria can be very important to fertilise semi-deserts.

VM: Yes.

JM: Because the enzyme which produces IAA (indoleacetic) is not killed by heat. Very important.

VM: So, is this enzyme and these bacteria used in industry to produce the hormone?

JM: I think so. It will be very important. Now I will make a...

VM: A patent?

JM: A patent, yes. So the (*indecipherable*) photosynthesis analysis examination of a new enzyme at that time (in '54-'55) was very important. In my future it was only a step. I was always interested by the study of the environmental process even as I see it in photosynthesis and also in cheese but also certainly in soil, soil fertility, because this plan interested IPSEA. And now after 50 years I see it is the information I obtained in Donner Laboratory. It is very bad that I don't remember the name of this young lady, Ph.D., which worked with Tolbert: blonde woman but I don't remember her name. And I study and was very interested by the ionisation chamber. And now in my lab. you will see this...

VM: ...ionisation chamber.

JM: I think the last one which works again!

VM: In the whole world?

JM: Yes. I have made a chapter in Solberg's (*spelling?*) *Chemistry* in English and American and I studied on the soil fertility and all kinds of processes like in desert or in forest and so on. It is always because I went six months in Donner Laboratory.

VM: So that six months in Donner sounds like...

JM: Changed all my life, all my life. And now we've heard Dr. Yen application because before I didn't know anything about cancer. She know but her friend Dr. Walon, who is also a woman, we have fresh breast tissue and it is our estimation that in two hours, even less, we can look the kind of cancer.

VM: You can identify...

JM: Yes. It is always because it is the culmination of the six months I passed in Calvin's laboratory in Donner.

VM: Well, I think that has had a most remarkable effect on your life, perhaps...

JM: *Alors*; it was (*indecipherable*) in life. Why I stay here? I see all my colleagues are already going away and I didn't. Personal (?) But now you will say if we can stay here even, we will look because I can synthesise also some protocatechuic acid — protocatechuic acid I synthesised by myself. So what I ,hope, what I think I will find it is really the correct label carbon dioxide substrate which is the key (..?..) to find (*whether*) this tissue will be cancer or not. We must look not when it is too late, before: when the tumour is there, it is cancerous or not. When we will find the right labelled substrate (*indecipherable*)...

VM: Which substrate do you use now for testing?

JM: I can show you. You will see, you will see, you will see. That's a good question. Now, for the future by radiorespirometry I study work in Melvin Calvin's. It is important for me not only to study the question about nature — fertility of soil — but in medicine. We think that by this very simple way we should make a new radiorespirometer and, with new radiorespirometer without making too many biochemical or enzymatic experiments like I show to you now only in very...

VM: Direct?

JM: Not direct; directly to see even in South Africa, even in Congo we can find the way to know this is cancerous or not.

VM: You want a simple technique...

JM: ...simple technique.

VM: Which you can use anywhere in the world, easy to train people, so they can discriminate the differences...

JM: And by the way of Dr. Yen, even we now have a Japanese laboratory interested by Michele Schultz (?). Now for you, if you know that, if you find because it is an apparatus with no dinacons (?), anything, no questions, no construction now.

VM: Yes, sure.

JM: I want to know, maybe in California or in England, I am very interested for the future now I want to can make it a new radiorespirometer with (*indecipherable*) method...even in England or in California or even in Taipei, I don't know. It is very important. There is only one which works and it is very important to do this. It is a question of great concern because I can show to you now my biochemical process. It is very difficult to submit success, too much, too big, too complicated. In summary, all the scientific life, my scientific life changes because I went to the laboratory of Melvin Calvin and I read and I can apply some radioactive techniques in Calvin's. And this technique was in this book *Isotopic Carbon*.

VM: You have not said very much about your relations with Calvin personally. Did you have much opportunity to talk to Calvin?

JM: About this point, it was for him quite different from other people. I understand that. He was concerned...he and me, he don't say anything.

VM: Really?

JM: He was expectative (*had expectations?*). You understand. You are in the lab. You have ten people who work on algae and you have one Belgian. What do you say to this Belgian? He works on spinach!

VM: So he didn't understand your working on spinach?

JM: He was thinking I am crazy! But one year later...

VM: He realised.

JM: He realised.

VM: But did you have a good relation with Calvin when you were there?

JM: *Oui*, no problem, *pas problem*. He invite me in the family, eh?

VM: Sure.

JM: Sure.

VM: And so the few months when you were in ORL you worked with Andy Benson on the enzymes?

JM: No, the first three months I worked with Tolbert.

VM: Yes. But then you went to ORL.

JM: And after that I go to ORL.

VM: And you worked with Benson.

JM: With Benson; Benson take me in charge.

VM: Did Benson stay there the whole time you were there or did he leave?

JM: Yes, he leave the end December and I leave, I think, six weeks later.

VM: In January-February?

JM: *Oui*, yes.

VM: I see. So you worked closely with Benson?

JM: Yes, yes, very close.

VM: Have you been in contact with Benson since that period?

JM: Oh, sure, sure, sure, sure, *oui*. Even now.

VM: But you have not seen him in America?

JM: No, not in America but he has seen me one time. He has seen me at Louvain. *Voilà*.

VM: So, your relation with Calvin — did you see Calvin; did you visit his family?

JM: During this year I have received almost two invitations and I go and see his wife and two children and we have a good talking. It was very friendly with him, no problem.

VM: Did he ask...?

JM: *Attendez!* I must say also when we publish (*indecipherable*) he accept my point of view and when I was there any scientist as he accept I go six months in the Donner Laboratory. He knows also I find Professor Stanier and he accept also I am working on spinach. He accept my suggestions.

VM: Yes, OK. And I see that eventually, of course, he referred to your work in his paper and he also published together with you a paper.

JM: Yes. He accepted to publish my experiment.

VM: But it took him time to become interested.

JM: Yes, yes: no problem.

VM: So did you have...I see that you worked very hard when you were there. Did you have time to relax? Did you have time to go to the mountains when you were in California?

JM: With the family of the lab., eh? *Alors*, I saw only with persons of the lab.: Mount Lassen National Park and Death Valley, so...

VM: Yosemite?

JM: Yosemite Park, *mais oui*.

VM: So you saw something of California.

JM: And the Calvins was there — not every time. He was a very good...I don't know, he make seminars and he was always there and we had always good talking. He tried to people go ahead; yes, no problem.

VM: To encourage people.

JM: Yeah, yeah, encourage people, sure. Even I. I have a way that maybe he don't understand because for about ten years he work on algae and then people, strange people, talk about spinach. But he accept the results, eh? Because you see, eh? Papers on research; my three chromatograms are there, no problem.

VM: Did you find him...when you saw how he was the leader of the photosynthesis, do you think he was a good leader of that work?

- JM: Yes, yes. For me I was not following Melvin Calvin, I was (*indecipherable*), not success. The success of my life was over (??) because I have a good relationship with Melvin Calvin and people know he very influenced my future.
- VM: Sure, sure. Well, I think that has been a very interesting story you have told us and, as you say, your contact with that book and with the lab. in Donner was a very important part of your life and you have told us something of how you have developed...
- JM: Even now that I am old the University and the faculty accept I am there. Madame Yano (*a colleague*) work with me, no problem. Not any difficulty.
- VM: Very good.
- JM: I keep this one; I have two laboratories — no problem.
- VM: Very good.
- JM: You will see: a grand (*large*) laboratory: my radiorespirometer is there; no problem.
- VM: Well, I think it remains only for me to thank you for your hospitality, for your interesting discussion and for your very brave attempts to speak English into a tape recorder.
- SM: Very successful attempts.
- JM: I should like, *mais* I have no money, to invite him (*you?*) so after one month I can speak English with him (*you?*).
- VM: Absolutely. Maybe we will find a way.
- JM: You understand my poor English!
- VM: You understand my poor French! Maybe WE will find a way so we will stop now and go and have a look at your lab.
- JM: *Eh bien, voilà.*
- VM: Thank you.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Jacques MAYAUDON

Date of birth May 30, 1924 Birthplace ROUEN (FRANCE)

Father's full name Emile MAYAUDON

Occupation IMPORT-EXPORT TRADER Birthplace NIVELLES (BELGIUM)

Mother's full name Andrée REULARD

Occupation housewife Birthplace ROUEN (FRANCE)

Your spouse Suzanne VAN DORSSELAER

Occupation Housewife Birthplace BRUSSELS (BELGIUM)

Your children Puce (Martine (1959); Veronique (1960);

Béatrice (1962); Jean (1963); Marc (1965); All of them have a

Where did you grow up? In my family in Brussels ^(University degree)

Present community idem. All children are away but keep contact,

Education Scientific humanities in St Augustin College (1944) ^(with us)

Ing. Chimiste et des Industries Agricoles (1948); Dr Sciences

Occupation(s) agronomiques UCL (1954); California Univ (Calvin) (1955)

(Professor in Faculty of Agronomy since 1966 (UCL))

Areas of expertise _____

Microbial, enzymatic and catalytic measurement
activities by using radiorespirometrical analysis.

Other interests or activities In medical field with coll.:

Evaluation of the aggressiveness in human breast
cancer tissues by radiorespirometry.

Organizations in which you are active AIALV Member.

AIALV: Association of Engineers of the Faculty of
Agronomical Sciences of Louvain Catholic University

Chapter 53

CHRISTIAAN F. VAN SUMERE

Gent

June 1st, 1997

VM = Vivian Moses; CVS = Christian Franz van Sumere; SM;
CIVS = Claudine van Sumere = Sheila Moses

VM: This is a conversation with Chris van Sumere in Gent the 1st of June, 1997.

So Chris, it has been a long time since we have seen you and I have forgotten much of your story. What was your early education and how did it take you to California?

CVS: Well, I studied Chemistry at the University of Gent starting at the age of 18 (1947 it was) and it takes four years in Belgium, two years Candidate and then two years for what we call Licentiate. This is a different title from what you have as Master in England but it corresponds to the Master's degree, the four years. This was actually in classic chemistry. I was not allowed even to take a degree in biochemistry. I finished even in my Master's job, you know, in the laboratory of inorganic chemistry. But I wanted to do biochemistry so I took my Ph.D. in that department working on several splitting enzymes, celluloses and hemicelluloses because I got a grant from the Brewery Council in Belgium and they were interested in these enzymes because they are very important in malting and so on.

So I was right away in the field of plant biochemistry, so to speak, and later on, you know, I returned many, many years later. But then I thought, well, after this Ph.D. don't forget we came out of the war and it might come as a shock to you but even certain of the compounds (*nur für Waffen SS*) I have used during my Ph.D. because the Germans left Gent the 3rd of September, 1944, all at once like that, and of course they left marvellous compounds from Merck behind so why should we throw them out and not use them? But this gives you the idea in what conditions we had to work. We came out of the war and many things were destroyed. We had very little to work with and we had very old equipment and so on and so on. And I thought, you know, this is not right. I must see that I get all sorts of modern things. The education in Gent was perfect, theoretically. And so I went to Canada with a post-doctorate grant, you know, from the Canadian National Research Council. But I forgot to mention that at the end of my Ph.D. I went already to Edinburgh to study with Prees (*spelling?*) on hemicelluloses. He was the Editor of the Journal of the Institute of Brewing. And he helped me also to obtain this grant in Canada and there we worked in the field of plant biochemistry.

VM: With whom?

CVS: Oh, I worked with several people, mainly with Ping Chu which was a brilliant Chinese, and I worked on the biosynthesis of glucosans in *Aspergillus niger* starting from pentose, labelled pentose.

VM: That was in Saskatoon?

CVS: Saskatoon. And then, you know, I followed the discussions there and, since we had been interested in germination, I heard that they had lots of problems with rust. You know that the rust, the *Ureda* spores, play a very important role because they germinate and the germ tubes enter the stomata and cause the infection in wheat. I noticed actually that the germination was very important there so I went to the discussions and said, "May I try a few experiments?" And I did and so we detected, in fact, the self inhibitors and the activators — because in rust spores we found coumarin as an activator but ferulic acid and other phenolic acids were inhibitors so these were compounds which controlled the whole thing. This has a bearing on the resistance of certain wheat plants also because some contain more phenolic compounds than others, and so on.

I did not follow this too much any more lately; I have been interested in all these problems my whole life through. But lately, as I said, now that I am in politics, funnily enough, I follow less the literature, the scientific literature, although I am still interested. We then worked also on the cellulose-splitting enzymes of rust. So the old mechanism of infection, how it proceeds and so on, we published three papers when in Canada. And then from there, you know, already from my Gent time, I knew about the beautiful work done in Berkeley by Calvin and his group and this was always a wish for me to go there.

VM: How long were you in Canada?

CVS: I was in Canada about thirteen months together with Claudine; we worked together and we were very, very good, you know, as a team. And then, of course, before I left Canada, you see, I thought to spend there one year, which probably was a mistake. Later on I thought to myself, well you would have stayed there better two or three years. But anyway I had made arrangements since I had no grant directly for the United States, because I did not know exactly when I was going to go there, I had already made arrangements with a Belgian foundation and that had promised me to pay my stay in the United States. This was a grant which allowed me to live — to survive not to become a rich man, of course. This was not necessary.

VM: So you made contact with Calvin when you were in Canada?

CVS: I think...this I do not recollect so well any more. I might have written him before but certainly from Canada. He asked me the classical things, you know — what was my background, what I had done, what I had published and so on and so he accepted me. And we thought to go both there first but then Claudine became pregnant and could not go to the Radiation Laboratory for obvious reasons. Because her father and mother were medical doctors, and unfortunately died a few years ago (they became very old, around 90), we decided that she would come home. Her father and mother had also studied abroad, you know, and understood the situation very well. And then I would bring her from Saskatoon to New York and then from New York I would go by bus and this was a lonely trip and I can assure you with mixed feelings I went to Berkeley. And do you remember I arrived there as an Eskimo, you know, clad like a...?

VM: No, I don't.

CVS: You don't remember that? You should not forget that we have temperatures in January when I arrived in Saskatoon of -42° below. It was very cold. And you arrived there in Berkeley and temperature was like spring weather, you know — 15° Centigrade. When I first arrived in the laboratory everybody thought I was a very strange man, you know, but after a few hours I was also running without any pullover and so on and so on, but, of course, I can imagine even in the centre of the United States it was very cold. They found I was a rather funny guy.

VM: So when you arrived in Berkeley you came by bus. Did somebody meet you from the bus?

CVS: No, I think I came on my own from San Francisco. You know the bus went to San Francisco. And then I took there another bus. There was at the end, you know, of.....what is the name of the street again? California or something? In San Francisco there was...

SM: A bus station.

CVS: A bus station. An old bus station.

SM: First and Market?

CVS: Yes. First and Market; Market — that was the name of it. And you know there was also the little train...

SM: F train.

CVS: Yes. I am not sure any more if I came by bus or by the little train because I went so many times to San Francisco that I can't remember any more, but I think that it was by bus. So, I walked up to the laboratory and I think, you know, after asking several people, I arrived. It must have been at Dee's place, or something. And anyway...

VM: Dee Lea Harrison, the secretary...?

CVS: Yes, we called her Dee. And I know that after my first visit to Calvin, you know, I have still the book and where he wrote a few notes in and he presented me and he said, "Well, you know, I explained things and we had discussions and in the beginning I will beat you probably 100% of the time but later on you will also win a few battles". I am not sure if I won many battles but anyway after a few months I could better discuss...

SM: May I ask which month you arrived in Berkeley?

CVS: I arrived in January, early January.

VM: Where did you live?

CVS: Well, Dee found me a place on Hearst Avenue close to Euclid. I do not know exactly...we went to visit the house a couple of years ago but I do not know the number exactly. It was close to Euclid and it was very handy in this sense that I could walk to the laboratory and even I did so at night and during the night and did little jobs for everybody because I was very close. And I remember in 1976, when

Claudine went with me to the lab., that Dick Lemmon said immediately, "Madame, he should not do any more what he did before. Now we have to bring...you know we have a shuttle bus for students from the library to that digs." He was imagining this park but I wasn't minding...2 o'clock...

VM: It was quite safe at that time.

CVS: It was quite safe and I went there a lot. It was sometimes lonely, I agree, but I did it many times.

VM: So when you first came to the lab. presumably you talked to Calvin about what you were going to do.

CVS: Yes, that's right. Actually I had been involved in three subjects. The first, you know, was one that I worked on under the guidance of Ed Bennett which is a very nice man and I like him very much. Actually I think it was the labelling of A and B and C and B and all this sort of things starting from labelled purines and pyrimidines. And I think it was just a sort of training course.

VM: And you did this synthetically or biochemically?

CVS: Biochemically. As far as I remember, you know, we had precursors, labelled purines and pyrimidines, and I fed them to algae and then tried to get out...But you see they changed rapidly the subject and, you know, I always thought it was a sort of training course I got. They were interested, of course, in these labelled compounds but there were other problems they were more interested in.

VM: Did you do this in Donner or ORL?

CVS: This was in the laboratory next to Dee's office.

VM: In ORL.

CVS: In ORL, yes. Close to the cyclotron. At the end of this office you had the cyclotron. I was working there opposite Helmut Simon, you remember?, and back to back I was standing to Mel Look who was a good friend and unfortunately died. As I told you he organised our last trip in July, 1995. It is not yet two years ago and six months later he died, unfortunately. We are very sorry for that.

And then the second problem then I got, you know, actually the one I have been working on maybe the most, was the effect of gamma radiation on the ability of algae to photosynthesise and to grow and this was a job I did with Ozzie Holm-Hansen and there is a report. From the first there is also a report but I can't remember exactly what the pages in the UCRL reports are but this one is UCRL Report 3830, June 1957, from pages 42 to 47. If you are interested I can give you rapidly what the meaning was, in a few sentences.

VM: Yes, sure.

CVS: Well, what we did was study the effect of gamma radiation from the cobalt-60 source uphill (*i.e. on The Hill*), huh? The effect of gamma radiation on the photosynthetic abilities of — and the viability, I should say — and the viability of *Chlorella pyrenoidosa*. The photosynthetic uptake of $C^{14}O_2$ was not very sensitive to radiation doses up to 1 million reps. However, growth was strongly affected by a dose of

100,000 reps and the algae started clumping, you know, like that, and did not grow very well any more so their genetic apparatus must have been hit very badly. Well, we tested always after irradiation the photosynthetic abilities and, as I have said, you know, up to 100,000 reps the photosynthesis went on. It is only from one million that it really was hurt. But, of course, also it decreased because the reproduction of the algae was not any more as it should have been. So actually the gamma radiation was hitting mainly the genetic equipment, in my opinion, so the reproduction any more of the algae but strange enough, you know, at least at up to 100,000 reps, you had still incorporation. This was remarkable. So we did it also right away, you know, after irradiation and then after cultivating them for a week, you know, under the classic conditions.

VM: So you must have learned the technology — the chromatography and the radioautography, all of that — you learned that from Ozzie mainly, did you?

CVS: Ja, I think everybody — Ed Bennett, and Ozzie; Helmut was also before me. You know, it was a family. I must say also Barbara Metzner helped me with these experiments. And I think that I have paid a price. You see that is from gamma radiation. Nobody is sure about it but I always, think, you know...They treated it in the University and said well, for the moment it is not dangerous.

VM: It is a slight skin....

CVS: Ja but they have taken it away.

VM: A slight skin tumour?

CVS: Ja. Well, no, not a tumour really but, you know, something that was different from the rest. I was sitting there...and this is a hypothesis I do not know. Was there a slight amount of gamma rays that came out of the equipment anyway?

VM: You didn't have a monitor?

CVS: Well, we had to hold the films in our hands and so on and they were monitors but, you know, as I said I have no direct proof. But it was the place where I was sitting there. So I think that this work had also a bearing on what could have been the effect of the dirty bomb. I think that was the reason why they were in those days interested in the dirty bomb, which was the atomic bomb which was then, you know, producing the hydrogen fusion. So, in fact, a hydrogen bomb and then with mantle of cobalt-60. This was the dirty bomb. And this was the question — what happens if this explosion takes place and you have this fallout of cobalt-60 and what happens to vegetation? And I think this was one of the reasons... I was always impressed when I used, you know, irradiations of a million reps and so on that in a few seconds, so to speak, the test tube turned black, you know. We produced holes, of course: we shot out the electrons, ions. Sorry if I use this poor way of putting it in English.

VM: No, that's fine. Was this not the time when people were taking swabs from the gutters? Do you remember Karl Lonberg?

CVS: Yes, I was looking for his name, Lonberg. The one with a black pillow usually...Ja, ja, that's him.

VM: He was taking samples from the gutters on the roof and finding radioactivity.

CVS: I think I have heard that but I must now go very deep in my memory. It does not sound strange to me when you tell this, you know, but I never gave later on any more thought to it. And then the (*indecipherable*) conduct, you know, perfectly well this was the effect of radiocarbon on the rate of carbon dioxide utilisation during photosynthesis. I started this work but you and Ozzie have made a good job out of it, you know, because you have made new experiments and so on and I am very grateful...

VM: Well, you really didn't have enough time to finish that.

CVS: Because I was already... You may say the first month more or less I had to find my way in the laboratory and then work on this first problem and we could label it. And, of course, if you would make enough chromatograms and get out all the spots, you know, AMP and ATP and eventually hydrolyse ATP or AMP and so on, then you would have had enough of it. If I may say something, I believe the idea was also that the AMP could be an acceptor of CO₂. Remember that?

VM: I don't but there were so many ideas...

CVS: There was once upon a time, you know, but again, you see, this was a long time ago, I'm not sure... But anyway I always considered that as the training course. Then the second one we have been able to do something about it and then the third was the Boichenko study. You know that and you have done a very good job and I was happy that something came out of it. We found already that it was not much of an effect. You remember that one day we thought also that the pipettes contained still some chromic acid and that the bicarbonate was split and, in fact, released CO₂ before it got to the algae, before we pumped it into the suspension, OK? And remember that I was doing these experiments and I had also the films in my hand with rather high concentrations... I mean high specific activities of P³² in little tubing that we put into the algae. These are the experiments that I was doing and so on and so on and then later on you have with Ozzie done a wonderful job and...

VM: But, not wonderful; we finished it.

CVS: Ja, I mean but I am grateful to you so a paper came out of it and I think that is the paper, *Biochimica Biophysica Acta*.

VM: Yes.

CVS: Vol. 28, page 587 to page 591 in 1958. And this is the paper: "The Effect of Radiocarbon on the Rate of Carbon Dioxide Utilisation During Photosynthesis," and the authors are Ozzie Holm-Hansen, Vivian Moses (I salute you sir!) and myself and Melvin Calvin.

This actually was then the end of the story because I think it was in July that I left Berkeley and then I travelled still through the United States. I think I came home... was it August or something?... because I went to see Alexander Hollander in Tennessee in Oak Ridge and I tried to visit, you know, important laboratories. I also went with the compliments of Calvin and Alexander Hollander to see Pomerat at the Rockefeller Foundation but Pomerat was out of the city but somebody, you know, anyway took notice from my request to get some money and later on Pomerat came to visit us in Gent, you know. You know the story how it goes: the man who had spent his last dollars to see Pomerat, you know, well he was not even asked by the chaps in Gent then to meet Pomerat and Pomerat had to ask, you know, "Where is Dr. Van

Sumere, I would like to see him,” and then I was called. You know how it is. And in the papers, of course, other fellows got the credit for the money we got from Rockefeller.

VM: You got money from Rockefeller?

CVS: Ja, we got something like around \$20,000...no, not so much...between \$10,000 and \$20,000 but I have never known exactly... there was equipment bought with it and so on? The strange thing is I never used that equipment. So I do still other things now for science without getting profit out of it, you know.

VM: To take you back 40 years: I remember that when you were in Berkeley your first son was born...

CVS: That's right.

VM: And there was a party.

CVS: Ja. There is also something in my mind on that party. That's the party that Ann Hughes gave, you know, organised, with the people from the lab. Because I have always been very grateful to Ann Hughes. Ann Hughes washed once a pillow of me, you know, and cooked a meal and took me to a concert and because sometimes I was lonely although I had many trips in beautiful California with the Simons and Chauzy (*spelling?*) Beauvais: he was a guy who worked with Arnon. And he was from Luxembourg and his wife was French and so on. And with were other people. The Metzners sometimes went also along — and you might have also. And we have been together to Yosemite. You know it was a whole family and Dick Lemmon was my ski teacher on my 28th birthday; I shall not forget it.

VM: Do you still ski?

CVS: No. The point is first of all I had very little time doing all this sort of thing and, well, you know, I have now left actually the subject. You were asking me...

VM: The party. What do you remember about the party?

CVS: Well, I remember the party and something very special. I was very casually dressed that evening so that two Englishmen and their ladies whom I knew very well — those Englishmen were Professor Vivian Moses and Sheila Moses and Professor Bob Rabin and Sheila Rabin — nicely dressed in smoking (*jackets*) and beautiful dresses the ladies and I, myself, just like a dock worker, or something like that. But anyway, this was a marvellous party.

VM: Have you not remembered Ozzie?

CVS: Oh, yes.

VM: Ozzie came in a white...

CVS: Oh, in a white...

VM: With a red cummerbund.

CVS: Right. I should not forget that. Yes, he was in a white smoking — he was really dandy. And I still remember that they had made a sort of crown, you know, with CO₂ and I think it was strawberries that were in it — a frozen crown, you know, and they dropped it in the punch bowl and it all started foaming and bubbling and smoking, you know; I remember that. It was a fantastic party and a great surprise to me.

VM: It just occurs to me: there was an interesting comment on the dress. I don't know where Ozzie got his dinner jacket from ...

SM: He had one.

VM: He had one. But Bob Rabin and I, not knowing American cultural practices, certainly not in California, naturally took our dinner jackets with us to America and this was the only occasion on which we had...we were able to wear them.

CVS: Claudine said "but he had it also also", you know, and I had been wearing it a couple of times in Saskatoon but then we cut it out and in California nobody has seen me with my dinner dress. But I found it a fantastic bunch of people, you know, and there are friends you can never forget. The funny thing is that, you, Vivian, told me always, "oh, those bumpy roads in Belgium" You were the only, in fact, who understood a few words in Dutch.

VM: Yes, well, I still understand a few words; it hasn't grown very much, a little perhaps, not too much.

CVS: And incidentally, you might recall that the bench part I had in Berkeley was the one from Nel van der Meulen who left the day when I entered so it was a Dutch colony all the time because...

VM: And you will be pleased to hear that we saw Nel van der Meulen on Thursday night...

CVS: Is that right?

VM: In Delft.

CVS: I have only seen and met Nel; we have been talking five minutes together, you know, in '57 and then I say, OK this was Dutch property, you know, Dutch people's property, and I will keep it again. But never met Nel any more.

VM: We can talk later about what she is doing.

Obviously you had, although you might have been lonely on some occasions, you clearly enjoyed yourself both in terms of work and social life.

CVS: I must say, this was the thing that kept me going. You might remember that I went with Bob Rabin for the first time through the west, ja? This was a very nice trip and I was very nervous at that time because of Claudine and Chris coming and I should also not forget Ingrid (*Fogelström*-)Fineman. Ingrid used to live with her husband in the same building as myself but then he left a bit earlier and Ingrid cooked once in a while some soup for me. It happened that we went together to San Francisco for a meal and I believe also with you probably to Chinatown and Utz Blass.

VM: Very possibly.

CVS: This brings me a story to my mind. Utz took us — it might have been with Ingrid and with Mel Look, I do not know exactly anymore — but he took us to ...

SM: It was with Mel.

VM: With Mel and Marie.

CVS: Ah. We went to Chinatown with different people once in a while and it was very cheap meal but a very good meal. We paid \$1 apiece and if we were with four people we got four different plates and if we were eight people we got eight different plates so there was always enough to eat. But one day Utz took us with his car, you know, to Chinatown and when we had finished our meal, you see, he realised he had left his keys in his car so he had to play kid, you know, like a burglar, and some of the people in the street didn't like that and were going to report to the police. As far as I remember (maybe I am imagining now too much) but we had to tell them, you know, that this is his car and the keys are there, etc. etc. And you know to open his car: he put a towel around his fist and knocked out one of the windows so I think he was a bit hurt also. There are all these stories, ja, you might have heard them already.

VM: Not that one and we saw Utz two weeks ago ...

CVS Ah, my God...

VM: But he didn't tell us that story.

CVS: It is a long time that I met Utz, yes.

VM: Many people we have talked to have been very impressed with the building, with ORL. What did you think of ORL as a place to work?

CVS: You mean the old building or the new one?

VM: No, the old one.

CVS: Well I think, you know, this was a very fine solution in this sense I suggested it also in Belgium. What is important is the equipment inside the building and the brains that are inside the building, you know? We went always the other way and probably because the reason for it is that the climate is not permitting like in California. We always put up, you know, a sturdy building that costs a lot of money and then, of course, we run sometimes out of money before buying the necessary equipment. After the war, now it has improved, but after the war you must understand, you know, we had to rebuild a lot of places. The country was destroyed, you know. Many cities were really destroyed. Also after the Battle of the Bulge and so on — and bombardments. So you must understand that we were not that rich, especially when I started.

VM: So you were impressed with the equipment in the building?

CVS: I think so, relatively spoken for that time. Of course later on we had all better equipment — scintillation counters and all that sort of things — but looking back, you know, all we needed was there, wasn't it?

VM: What about the structure of the building — the open laboratories, the lack of walls. Do you think that was a good thing?

CVS: Well, I will tell you something. I have used a bit the same principle in my own laboratory so I could walk, you know, from one — the left side: do you remember...the left-hand side, Claudine? — we could walk from the one to the end. It was a long...Oh, we can pass it, you know. It is a very tall building and I had the 7th floor and a colony on the 8th floor, and one side you could walk through from the beginning almost to the end with the exception of two or three rooms. So I thought that this, in fact, was not too bad an idea because it improved the communication and the speed, you know, with which you could switch around certain equipment. Would you agree on that?

VM: Indeed. And of course we used to meet all the time...

CVS: Around the table.

VM: ...around the table and compare results and so on.

CVS: We had this also and there are photographs when Calvin came and he got this honorary degree in Gent (1970) and he spent here with his wife, you know, a week, eh Claudine?

CIVS: Yes

CVS: And I forgot to look for this photograph, but there he is sitting with us around the table, although it is not such a famous table like in California where we were having tea around the table, where the prototype of the cyclotron, the first cyclotron, was standing on, right? Is my memory correct?

VM: Correct. But later on you went back to Berkeley and you saw the round building.

CVS: Yes.

VM: What did you think of the round building, bearing in mind that the old one had to be destroyed in order to make a new Chemistry Building? What do you think of it as a way of trying to recapture the atmosphere?

CVS: Yes, I think it was a good idea although, you know, I missed the cosiness of the old building. And of course, everything was...well, actually I am not going to say that I was a pioneer in the field but anyway I was with the pioneers and I thought it was a very pleasant time actually. I am not sure it had still the same spirit later on.

VM: Well, I think it is difficult to keep something going for a very long period, particularly when it gets very big, as it became later.

CVS: Because one day...you see I have been back in Berkeley, as far as I can recollect, in '64, '76 and then I went with about 45 people, Belgians, to the lab., you know — people from all walks of life. There were politicians, high judges and everything, university professors and I wanted to tell them and show them, look, this is the way the Americans do it and there history has been made, you know, especially in the fields of nuclear sciences. They were very impressed, eh Claudine? And we had a meal in the evening, ah, well, I think Dick Lemmon was there with his wife, Marilyn was there, you know, and Martha was there probably and Bennett. We had a meal, they had organised for us in the Mandarin. My friends wanted to have a Chinese dinner, top class. This was fabulous; I'm not going to say how many bottles of wine there have been but it was over fifty, eh Claudine?

VM: Fifty bottles of wine?

CVS: Ja, ja, ja. But the Belgians were in such a mood, you know? And there was also a German lady who had been in the lab., Luise.

VM: Luise Stange.

CVS: Stange, ja.

VM: We saw her...

CVS: My god! You know, I never met her, you see, but you know we called ourselves the Calvinists sometimes and I said, "Come on, Luise, you come with us for a meal" and then they had to drive her, you know, in a hurry to the airfield, because she was leaving that evening. In the same way, and I am thinking he unfortunately died also, was George Akoyunoglou.

VM: Yes, he did die.

CVS: Ja. So we never met in Berkeley but we knew from each and other that we had spent there part of our life, you know. I had a lecture once in Israel and I stopped in Athens and Georgie took care of me and when he came to Gent I took care of him. So it was actually an international organisation; it even worked for those who were not together in Berkeley. And I think this is something that I, for all the gold in the world, would never like to miss.

VM: So it clearly has had an important part to play in your life.

CVS: It has had an enormous...For instance, look at it. I am going to tell you it very briefly. First of all, you know, we got some money via the Rockefeller, via Berkeley, via Alexander Hollander, who was also extremely nice to me in Tennessee. And then next to that I taught, for instance, for scores of years a course in radiobiochemistry. I would never have done that. Of course I had been working with isotopes in Canada but I would never have done it, you know, if I had not had the further experience in Berkeley and then the book from Calvin and, you know, all that sort of things and the contacts with other people. I went also to Munich for the gas phase determination of isotopes with Helmut Simon and so on, and also to Saclay. But I think that Berkeley, actually for my "tracer life", this was the most important.

VM: Of course you were much younger then, more impressionable and it was an important time to learn things.

CVS: Oh, I think so. And you know also, what I liked very much and I saw the same spirit with Dick Synge later on; I don't know...Dick Synge was a very great friend of mine. This was the humility of these people, from these Nobel Prize winners. And actually Calvin also...I might remind you again: "you might lose in the beginning but later on you will win a couple of battles when discussing with me". This was something that you would not directly expect, you know, from this kind of person. So this humility in science and I have also tried to foster that also in my people, you know. Then, of course, the contacts, you see, when I was teaching photosynthesis (I also gave a course in photosynthesis); I got the most recent chromatograms from Berkeley. When I went there, you know, Martha made them for me, improvements and so on, you know, so I could show them.

- VM: So you kept in contact with really quite a lot of people one way or another over the years.
- CVS: Oh, ja. Who has been? I think Ed Bennett has been several times in Gent; Martha has been in Gent; Calvin, of course. Then you, Bob Rabin, Bob Rabin has been here in this house also and Helmut Simon, who also was there, you know?. Janet, the girl from Australia — she wrote me once when there was a congress in Brussels on photosynthesis but I forgot her name...
- SM: Anderson.
- CVS: Anderson, that's it.
- VM: We are in touch with her and we may see her. She is in Australia but she comes to Europe...
- CVS: Please give her my regards. When flying over Australia — we went to New Zealand, you know, for a cruise and so on in January and February — when flying over Australia I thought, where is she now?
- VM: In Canberra.
- CVS: In Canberra. Because through Dick Synge I was once upon a time also an external professor — well you can't say the words "external professor", it is too far fetched — but at least I was in permission of Ph.D., you know...
- VM: External examiner?
- CVS: External examiner, that's the one I wanted. And this was Dick Synge and it was more or less...was on the basis of that.
- VM: Where was he?
- CVS: Dick was in Norwich; when he started, of course, his work in Aberdeen.
- VM: Oh, this is Martin and Synge!
- CVS: Ja, sure. Dick, oh, we were very good friends. This came very strange. Once upon a time I published a paper in *Phytochemistry* on proteins that contain phenolics and so on and this is the reason why Abro (*spelling?*) and I have also published this book, you know — edited it, of course. Dick was interested in that and wrote me a letter and said, "you know, that if you allow me one day in your laboratory I will come to your meeting in Gent". Oh, this can't be Synge, you know, who asked to stay one day with me in the laboratory. So I asked Helga please check that name and she said, "Professor, he is the Nobel Prize winner." So we actually became very good friends, eh Claudine?, and we have been travelling to Britain and travelling here in Europe together, you know. He was maybe in many ways the opposite from what I am...
- VM: When you came back from Berkeley, in '57 I guess it was, did you have a job already in Gent waiting for you?
- CVS: Yes, more or less: they did not want to let me go and I got something from the Patrimonium from the University but not really an appointment so I made very little

money in the beginning. And then I could tell you stories on that so Claudine kept me going by our pharmacy. And then in 1961 I became an Associate Professor. In the beginning I had to start from scratch: with two desiccators I made paper chromatography. And I had to start from scratch.

In 1959 I got some money for buying the first tracer equipment and slowly but surely I have been building everything up. And in 1965 I became a full Professor and then I taught biochemistry and enzymology but, mind you, I was already teaching at the University from 1958, January 1958. My boss, you know, became the Chairman of the Scientific Committee in Belgium so he was many times in Brussels and I gave his lectures and these lectures I have given from '58 up to 1993, you know.

VM: You retired in '93?

CVS: Ja, 1993. Actually, the last year I did not lecture too much anymore because I was busy with as head of department and all the changes that took place and so on.

VM: So really the whole of your life, from the time you came back from Berkeley, has been based in Gent?

CVS: Based in Gent. But as I said with NATO grants I went to Munich, I went to Paris. I was at a certain moment involved with the Phytochemical Society (*indecipherable*) in England and then later on I became the Vice Chairman, Chairman of the Phytochemical Society of Europe. This may be a strange thing to you — I was the first non-British.

VM: Were you?

CVS: Yes. So more or less they accepted me. And it was during the week that I was staying with Dick Syngé in Norwich to finish a paper that they elected me in London so I did not manipulate anything for that. I have been mainly involved in phenolics, the biochemistry of phenolics. I have written maybe, together with the people working with me, three papers on algae. You know, 1 on the photophosphorylation and the uncoupling and so on but not too much because I thought you can't compete with the big shots, you know! You can't do that. So I had this problem already going from before Canada in Gent and then Canada, you know, the phenolics and so on and there, of course, I have been very active. And as you see this is also phenolics and later on, you know, this is again, you see.

VM: These are books that you...

CVS: Ja, and the one with Tony Swain and Jeffrey Arbon that has been published in *Plenum* but was not well looked after. Tony had an accident and something went wrong there. And, of course, what we did, and this is the reason we won several prizes in Belgium — they asked me one day to develop a technique for the protection of our Azarea (*spelling?*) cultivars which bring a lot of money to Belgium and to this region and we produced something like, you know, I dare not say it for the moment, but anyway 90% of what we produce in Azarea is a lot — millions and millions of pot plants — it is not too far from a hundred million, I think. This brings millions of Belgian francs to Belgium and the competition is very hard with the United States. This is, of course, because they have their own territory but from Germany, the Netherlands, France.... And so we have very beautiful cultivars and, you know, we make new ones by cobalt irradiation — cobalt-60. It is very strange that I...

VM: And who is the expert on cobalt-60?

CVS: Ja, but I did not do that anyway. You know, their own expertise was built up here in Belgium but I understood it very well. But they asked me to Brussels and said, "Listen, we must be able to patent the plants but what happens. We have put a lot of money into the production of new cultivars, new colour varieties and then we see that they are produced *en masse* in other countries and this is something that we can't keep going because we are investing our money and the others get the benefit". So what I did was, and this has been improved later on a lot, you know, was making fingerprints. This is the beginning only; we have much (*indecipherable*)...much nicer fingerprints from all the phenolics. Then we translated that by a computer programme into an electronic pattern. And here you will see, for instance, *niobe* is a white one and the unknown (*indecipherable*). So you can say, hey, hey, you took that, and we have done that for 600 about ...

VM: Ah, so this is a way of fingerprinting your flowers in order to protect your rights on those flowers.

CVS: Yes, you see, so the fingerprint is converted into what we call an electronic passport.

VM: Yes. And it's based on an HPLC analysis of the flavenoids?

CVS: Sure, yes. We have them for about, oh, I can get them rapidly upstairs, for about...don't call me a liar when I say around 600 or 570 or something like that, you know. Now, this one is the prize for the best agricultural research. This is a 5-yearly prize so every five years they give it in Belgium. And this is...

Tape turned over

So for the period 1982-1984, for the five years, we won the prize, the Müllee (*spelling?*) Prize as we call it, for the best agricultural research because this is also considered, of course, horticulture. And we did the same thing for hops so we can now identify even hops even when the cones are ground, you know. People thought they are never going to be able to do it. Now we made fingerprints and we could nicely see when it was a (*indecipherable*) and when it was a Brewer's Gold, which is much cheaper.

VM: And this is also based on flavenoid derivatives?

CVS: Ja, the flavenoids, those that are mainly (*indecipherable*) derivatives on which we were basing ourselves and so forth and the camphor derivatives etc., etc., not phenolics. Because this was so valuable, you know, the (*indecipherable*) said we are going to kill him and the brewers said we will erect monuments to him and I said you better send the cheque to my wife! Then I got in 19.., oh, when was it again, in 1986, no 1988 it was, I got the 2-yearly prize from the Brewing Convention, the Belgium Brewing Convention, and then also the I won the van der Stricht (*spelling?*) Prize for that. So we have done it for roses now. Of course, it is very important. The French were also very interested in that, I can tell you stories about it. So I have done a bit of everything but all in the field of phenolics.

VM: Well, that's until you...and you retired in '93 when you were 64 years old, you said?

CVS: Yes, 64.9. And I should tell you, you know, the story which is maybe very important. That is in 1983 the Belgian flaxers came to see me and said they needed a very quick

retting system because we are losing our grip, you know. Can you? And I said, "Well, I have never had a flax plant in my own hands, you know, and you ask me something. Let me think for 24 hours." So I did that and then I developed an enzymatic retting technique and instead of retting in the pits, you know, with hot water for — depending on the quality of the flax and the change from year to year: the photosynthesis and the climate and all that sort of thing — but let's say from five to six days...seven days sometimes and even more...but around five to six days and it costs energy and so on. We do it in sixteen hours. Mainly they use a classic field (?) retting, you know, dew (?) retting also. This takes about plus/minus seven weeks. So we do it in sixteen hours and I get it out as white as that if necessary. So what happens is that Novo...the story goes it is very strange. The newspapers came to my lab. when I got this first prize, you know, the Agricultural Five-year Müllee (*spelling?*) Prize, and I made a newspaper article and the guy who was making the pictures said, "Flax, Flanders flax!" So this came in this newspaper and the scouts from Novo Nordisk read it and said, "Well, we are also interested in developing enzymes and so on. Could we collaborate? This is what you need and this is what we did." then, of course, Novo said that people all over the world would know that I was following and you have an advance in all this and if you agree we are going to patent it. And this is what I did and this is from Novo Nordisk. There is a black spot and they wrote on this paper. And, of course, because it had to be desalted. This is, you see, something that I had by accident. So many things we finish here because people came to ask me.

VM: Sure, that's the nice way.

CVS: And this was, again, on the basis of several splitting enzymes as what I told you earlier, my Ph.D. and Canada and then I left it more or less and at the end they brought me back to it.

VM: Full circle. Just to finish, but very briefly, since you retired from the University I understand you have a new career...

CVS: That's right.

VM: ...in public service, political activity?

CVS: Political activity — actually the Flemish Liberal Party. The ex-Vice Prime Minister of Belgium, Giverofstaat (*spelling?*), who was the Chairman of the Party then, invited me to join the Party and to play, still, if possible a role. Of course, you do not start a political career any more unless you are Adenauer or Churchill or Reagan, maybe, at the age of 65. So I said well I can do still a job maybe locally, so they suggested that for me the province was actually like a senate, you know, where people speak in a soft way, nicely and respectfully to each other. So I am doing that and there I fight for science.

VM: Are you a Senator?

CVS: No, no, a Provincial Councillor. I represent, actually, 17,000 people plus/minus.

VM: And you fight for science?

CVS: I fight for science. And I got already twice a subsidy for the University. And now this time I hope, and I think people are getting convinced now, it will be a more substantial investment because I give them all this proof — you know, look what

happens. For instance, when you look at the gross national product, Japan has invested 2.7% in science, America 2.5, Europe as a whole 1.9, we in Flanders 1.85 so we are just at the middle of that of Europe but still under — we must, you know. So that is what I am doing, mainly. Other things as well.

VM: Good. So that has taken us a long way from Calvin but it has been very nice to meet you again and to hear your memories of those good old days.

CVS: Yes; I thank you very much and I am so happy that you are here, you can't imagine.

VM: We are equally happy.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name VAN SUMERE CHRISTIAAN, FRANS
Date of birth February 10th 1929 Birthplace BEERSEL (BRABANT).
BELGIUM
Father's full name VAN SUMERE SEBASTIAAN
Occupation ENTREPRENEUR Birthplace LINKEBEEK (BRABANT)
BELGIUM
Mother's full name WOUTERS JOHANNA
Occupation Haute-couture dressmaker Birthplace BEERSEL (BRABANT)
(BELGIUM)
Your spouse DE PRETER CLAUDINE, MARIA
Occupation PHARMACIST Birthplace NOVEMBER 3rd 1930
Your children CHRISTIAAN JR, ANN, PEGGY, KOENRAAD

Where did you grow up? BEERSEL (BRABANT) - FLANDERS (BELGIUM)
Present community G HENT (EAST - FLANDERS) BELGIUM
Education ph.d Chemistry (1955) and D.Sc Chemistry (1963) - Univ. of Ghent
Studies abroad: Heriot-Watt Univ. Edinburgh (UK) 1953 (Belgium)
Post doc. fellow NRC Canada (Saskatoon) 1955-1958 - Univ. California
(Lab. Prof. Melvin Calvin (1957); Univ. Munich 1960; Centre d'etudes nucleaires
Occupation(s) de Saclay (France) 1963

Emeritus Professor Univ. G HENT - Former director Lab. Plantbiochem.

Areas of expertise BIOCHEMISTRY - PLANT BIOCHEMISTRY
Radiobiochemistry - Enzymology.

Other interests or activities Elected Councillor of the province
of East-Flanders; scientific policy, education, economy,
arts, history and literature

Organizations in which you are active Flemish liberal Party.

Chapter 54

LISE (SCHOU) WILKINSON

London

June 14th, 1997

VM = Vivian Moses; LW: = Lise (Schou) Wilkinson; SM = Sheila Moses

VM: This is a conversation with Lise Schou Wilkinson on the 14th of June, 1997 in London.

I wonder whether I could start by asking you what your early training in science had been and, therefore, why and how you found yourself in Calvin's lab.?

LW: I started actually with a degree in pharmacy and from there on I got interest in pharmacology and pharmacognosy and decided to do a PhD in plant physiology which was, you know, slanted towards finding what was in these plants, anyway, this sort of thing...natural products.

VM: Where was that?

LW: It was all in Copenhagen. In between...after two years of this, I had the chance of going to the States. I got a grant and it was actually a choice between Chicago, at that time, or Berkeley and Calvin's lab. Obviously one knew that California was, at that time, the promised land so one decided that maybe Calvin's lab. would be the better option. So, I went there.

VM: Did you know anything of his work at the time that you went?

LW: I had read up on it obviously before. One did know because those were the great days, when it was all bubbling up and the path of carbon in photosynthesis and all this sort of thing was *the* thing so one certainly knew about it. But, as I say, they were doing some of that in Chicago at the time but obviously...

VM: That was Gaffron's group in Chicago that you were thinking of?

LW: Yes...but obviously this was, you know, the nicer option, climate-wise.

VM: So, what did you do: wrote to Calvin and said could you come, essentially?

LW: Yes...could I come and, you know, there was no problem. They were very kind. It wasn't a lot of money one had in those days. It was '49 when I went in September '49. It was at that time very much the promised land when you came from war-starved Europe and so on. It was great. Everybody was very kind and I was

immediately put onto looking at algae and the usual...you had that very nice wall chart there...what it was all about.

VM: When you arrived in Berkeley, how did you travel, incidentally? Did you fly in those days?

LW: No, I went on one of the *Queens*, I think, one of the old ones. There was a sort of party of students there actually,, not necessarily going to Berkeley, but going all over the place. Some Norwegians, some Danes and some Swedes. We had a great time going across. And then I think I flew across America, stopping in Kansas City where I had a pen friend which had been arranged by the then-American Ambassadress in Copenhagen. So I stopped in Kansas City which was relatively interesting, but not the place one would like to stay very long, and then I went on to Berkeley. Where again, I found a room in International House which, of course, was a very thriving institution; well, of course, it still is — it was at that time. It was as I said full of mature students, shall we say, many of whom had been in the war or who had been held up by not being able to go abroad and this kind of thing. It was a very fertile environment at that time, both there and in the Old Radiation Lab.

VM: When you got to that lab. for the first time, you hadn't actually met any of the people, had you?

LW: No I hadn't. I came completely innocent of any...

VM: This was your first contact, was it, with American academics?

LW: Yes. In fact it was my first contact with anybody abroad because, you know, one hadn't been able to travel at that time at all. I suppose I met in my home before the war people, but that was very different because one was really just a child then and it was really purely sort of on a quite a different level.

VM: Your background had been academic, your father had been an academic?

LW: Yes, my father was an academic. In those days, of course, in Copenhagen...you know, it's such a small country and such a small even academic community everybody knew everybody else and it was sort like an extended family almost.

VM: When you arrived in the lab., do you remember what it was like, coming there? Do you remember whom you met?

LW: One was somewhat in awe of them all but they were all very kind. There was Vicki (*Haas*) as I say who unfortunately died quite a few years ago, Dick Lemmon and Andy Benson who was always terribly kind and nice. In fact it was a very — shall we say —jolly company actually. We used to do things like go down to Yosemite and ski. I remember I have never been so cold in my life, even in Scandinavia actually, as we went just after Christmas, I think, and took something like...well, they weren't cottages in our sense but they were sort of...

VM: Cabins?

LW: Cabins! That was the word. And there were these beds which obviously hadn't been slept in, you know, for all winter. Sleeping bags, yes, but you had to put on all your ski clothes as well and you could still feel the cold coming up from underneath the

mattresses. But it was great days and we enjoyed it very much. Quite apart from the science, of course.

VM: How did you decide on what you were going to do? How was it worked out?

LW: Well, I mean, you know...having read some of the things, and they said was anything I'd like to do and having explained what they were doing at the time. I said, you know rather timidly because I really was such a novice, you know, in this field, I said, you know, I'd ready something about glycolic acid and I'd like to, you know, explore its function and its place in the path. They said "yes, why not, that was a jolly good idea" and put me on, showed me the algae, the chloroplasts and all the rest of it, and I took it from there and went on with it. They were all very helpful. Because after all, this was a completely new technique for me and so on and one had to work one's way into it but everybody was very helpful. So, you know, in the end, one got a paper out of it.

VM: Who was there when you were there, apart from the permanent staff — Lemmon and Bassham and so on. Who were the transients?

LW: Well, there weren't that many at that time actually, but certainly the one I remember most is Cyrias Ouellet from Canada who was, of course, much older than the rest of us actually but who was a very nice chap and full of little anecdotes and fun and also helpful, you know, although he was doing something else from what I was doing but he was a nice chap. I can't remember...Otherwise, it was mostly the permanent people that I remember, I think.

VM: What stage had they got to in the photosynthesis story at the time when you were there because you said you got there in '49.

LW: Yes.

VM: That was fairly early.

LW: It was early, yes.

VM: They were into paper chromatography, I think.

LW: Yes, very much so. That was really the sort of staple technique. And we did that: we ran everything through the columns there and then pinned down, you know, the various compounds to see how far we had got in the sequence.

VM: As you remember it, was as there a fair understanding of what the path of carbon was at that time or was it very hazy still?

LW: No, I think they were getting to grips with it really, you know. One knew already quite a lot. There was lots more to be done, obviously, but I think the basic sort of understanding was there, what one expected to find actually, you know, which wasn't always what one did find, mind you. There was an understanding of the structure, I think. But, of course, Calvin himself was very good at running ahead of his results and getting rather fixed ideas about what he expected to find and sometimes, you know, finding things through his expectations, rather funny results there.

VM: Was he wrong much of the time?

- LW: No, not much of the time, but he was once or twice I think.
- VM: He would look at the data and really tend to extrapolate beyond the level that many of you would do.
- LW: I think that's the way to put it, actually. And some of the time he was right and some of the time he was not. But he hated to admit if he was not right!
- VM: Yes, indeed. I think one of the people who may have been contemporary with you was Bill Stepka who actually introduced the group to paper chromatography as I remember.
- LW: Yes, he did indeed. I mean he was there when I got there and stayed there while I was there. I'm not quite sure of his exact dates. Yes, that's right; of course I should have mentioned that. He was actually already so established there that one might almost think he was permanent, you know.
- VM: It is very difficult to know, to place people in the hierarchy before you...
- LW: ...when you first get there.
- VM: So when you were there, as paper chromatography must have been fairly new at that point, were all the spots identified or was there still a lot of time spent in identifying these things on chromatograms?
- LW: Many of them were identified but obviously some weren't and some time was spent, but I wouldn't stay a lot of time. Except, of course, new things could turn up and one would have to look for that. One had a pretty good idea; I'll have to go back and look at my slides, I think, to see just how far we had got by the time I left.
- VM: Who were you working with? Who taught you the ropes? Who discussed with you what you were doing and who was your closest collaborator?
- LW: I think it was primarily Andy Benson and also Vicki, as I said, the late Vicki, whereas people like Dick Lemmon I've come to know much better later, you know and that sort of thing. But the Basshams were there too.
- VM: Al Bassham was in the lab.?
- LW: Al Bassham was in the lab. at the time.
- VM: So you started working on glycollic acid.
- LW: Yes.
- VM: What were you looking for — or at?
- LW: I was looking for its role, really, and where it came in the general scheme of things, you know. It's all so long ago I'm ashamed to say that some of it is pretty hazy.
- VM: Do you remember where you got to with it?

- LW: Well, again, precisely I think I'll have to go and check the papers! I should have done that before I came but I did actually look yesterday but couldn't find them, I'm ashamed to say! Yes: I'll have to mug up on that.
- VM: How long did you spend with them?
- LW: Well, it was the academic year really, you know, from September until the following June, I suppose.
- VM: But only one year?
- LW: Yes.
- VM: And you spent essentially all your time working on that topic — glycollic acid?
- LW: Yes, that was so, really, enough to keep me busy then.
- VM: I gather that while you were there you met your future husband?
- LW: That's right. In International House. It was a great marriage bureau at that time. In fact, we still have friends here, you know, who we met there, who were there at the same time, who also stayed married, which is quite remarkable these days. One is the former vice-chancellor of Essex University and the Sheradian Professor of Botany at Oxford, Bob Whatley...I don't know...who was with...
- VM: With Arnon; we talked to him already. So what was Geoff (*Wilkinson, Lise's late husband*) at the time? Was he a student or a postdoc.?
- LW: No, no, he was a postdoc. You see, he has quite a long history in North America, actually, because he was sent over during the war with the atomic energy project as a very raw Ph.D. being hurried through in wartime in two years because, you know, they had to get them out there. He was sent out...in Chalk River he spent most of his time... with his fellow not only Yorkshireman but from the same town in Yorkshire actually, Cockcroft. Even not only from the same town but they actually attended the same school in (*indecipherable*) although Geoff obviously some years later. He always said that he had the same physics teacher as Cockcroft had had but by then the teacher was considerably older and not as inspiring. This is why Geoff didn't finish up with physics but with chemistry.
- VM: So they were both in Berkeley at the same time?
- LW: No, no, not in Berkeley: in Canada.
- VM: I see, yes.
- LW: Yes, the atomic energy project. And then, when that came to an end at the end of the war, Geoff thought he would like to have a look at Berkeley. Like lots of people, he wanted to have a look at California. So he travelled down there. He was the first foreigner to be cleared by the Atomic Energy Commission and he went to work on The Hill in Berkeley and stayed there, well actually, until the year I left, in '50. He had decided by then he had been churning out isotopes up on The Hill and decided that there was really no future in that. He would like to go back to more sort of chemistry.

VM: Was he actually an employee rather than a postdoc.? What was his status?

LW: Yes. He was an employee at that time, actually...yes, and he said at the time he'd never been so well paid! He went to MIT but he stayed there for, well, two years and then he went to Harvard.

VM: What did you do?

LW: I went home and finished my Ph.D.

VM: "Home" was still in Denmark?

LW: In Copenhagen, yes. Then we got married in '51; I came with him...the year he went to Harvard then. We spent the first five years of our married life there until his old teacher's job at Imperial College came up; he was retiring. Geoff went from Harvard for an interview in London. It just so happened it was just about the day our second child was born, but he went anyway. He got the job because he had always wanted to come back to England and this was obviously a golden opportunity. So we went back, coming to London where it was difficult to find a place to live and, you know, in many ways, we almost turned right back again. We persevered and have been here ever since.

SM: Which year was that?

LW: Geoff's appointment started from January 1956. And then, you know, he went on until he retired in 1988 when, of course, as in so many cases here, they had difficulty in finding a successor because these days young up and coming people can't afford to go to London where the cost of living of houses and schools and everything is considerably higher than elsewhere in the country. So he continued teaching for another three or four years until they finally appointed somebody who immediately went back to Texas (an Englishman). Again, they were without somebody but finally they appointed Mike Mingus who's there now. But Geoff, having been lucky in his career and his industrial relations, actually was provided with a lab. as he used to say "the finest penthouse in London" with a wonderful view and he was there ever since, still having postdocs. and graduate students and so on, a nice little group up there in his own lab., until the day he died.

VM: When you got married — presumably you got married from Copenhagen and went to Harvard — did you work in science in Harvard?

LW: Yes, I did actually. I was going to ask you earlier if you ever knew Kenneth Thimann...

VM: The name, not the man.

LW: ...who was doing growth hormones in plants and all that sort of thing. I worked for him for a while, until our children were born and so on and I had to mind the home business. Those were good years too and, of course, Thimann was also originally English but spent his last years in Santa Cruz; he stayed in the States but he was at Harvard for a long time and then went to Santa Cruz, I think in semi-retirement. In fact, he just died a few months ago.

VM: You then presumably came back and raised the kids and so on. But eventually you adopted a new career, didn't you, which was not experimental science?

- LW:** It was history of science, really, which I have been doing for more than twenty years and enjoyed it very much, straying into viruses and infectious diseases in general and lately tropical diseases which I have learned more about tropical diseases than I really want to know.
- VM:** You've done this a lot in association with the Wellcome Institute in London?
- LW:** Yes. I do, in fact, still have a little room there and spend a lot of...Because it is a wonderful library and, of course, being in London you are lucky because you've got so many libraries you can consult and so many archives that you hardly ever need to go anywhere else.
- VM:** Can I take you back to Berkeley in the late forties, early fifties? Two questions, really: Many people have talked to us a lot about what they saw as the influence of the building itself, ORL, on the way the group developed. Do you have any thoughts about that? Did it strike you as being a particularly suitable building for those people? Do you think it affected the way people operated?
- LW:** Well, it may or may not: I don't know that I have thought a lot about that. Of course, I was such a neophyte, really you know and I just, you know, admired the people there, admired the way they did things. I don't think...I mean it was...perhaps it did pull people together in a way because it was a fairly limited space. You lived very close together and you were thinking about the same things, obviously. Perhaps it was a fertile ground because you were almost forced to speak to people all the time rather than, you know, if you were in a bigger institution, people may be on different floors and all that sort of thing. But here you were really at the centre of things all the time in a sense. I think that's as much as I can say about that.
- VM:** Do you remember the time that you spent there as being a very lively scientific time?
- LW:** Oh, yes. Very much so.
- VM:** Was Calvin a participant in these discussions with you?
- LW:** Yes, he was. One remembers him as being almost always there. This may be an exaggeration. But certainly in person nearly all the time and in spirit certainly all the time. His ideas sort of permeated everything, I think.
- VM:** He wasn't in any sense remote to the bench people?
- LW:** I didn't think so, no.
- VM:** You had access to him to talk to him easily...
- LW:** Oh yes, you could when you wanted to, certainly. There was none of what — well, of course, I came from that European tradition and what Geoff always called the "Herr Professor" syndrome, you know. This was a revelation coming to...I mean, in the States in general at that time, it was, you know, first names, you know, and a very different approach on a personal level.
- VM:** Have you been back there to Berkeley?

- ~~~~~
- LW:** I have been back on a number of occasions. I particularly remember the last one when we went, I think, at Calvin's instigation actually because they were trying to interest Geoff in a job in the States or in Berkeley. And I particularly remember Calvin had a wonderful party for us in the garden there. What amused me very much was (I can say this now, of course)...It was a big party but Calvin had us and one or two others in a special corner because he had a special bottle of champagne that was given only to very special guests whereas the rest of them were in another part of the garden where they were served with some plonk; you know, it was not so important. But it was a very nice party and they were all very nice.
- VM:** Have you kept in touch with the people that you knew from those days?
- LW:** Well Andy Benson and Dick Lemmon certainly over the years. The rest of them not so much. This has been, you know, regular Christmas cards and so on. Actually, Andy Benson...I lost touch with for a few years and then he suddenly wrote because he had seen a picture of us somewhere (*Chemistry in Britain* or something, you know) and wrote and said "hallo" and so on and then we have been corresponding again in recent years.
- VM:** On your more recent return visits to Berkeley, did you go into the round building which was?
- LW:** Yes, I did.
- VM:** You know that was an attempt to convert into a modern environment some of the flavour of the old ORL. Do you think it worked, from what you could see?
- LW:** I don't think I really was there long enough to say whether it did or not. You are probably a better judge than I am on that.
- VM:** Well, I have my opinions but not necessarily yours.
- LW:** What I remember was Calvin's remarks when he had to retire and let somebody else sit in his office and that was not easy, obviously, as indeed it probably never is when you retire and have to accept a successor to what you have built up as your very own environment. But at least he still had Marilyn.
- VM:** Yes. So I think really the last thing I'd like to ask you is what your experience in that lab. did for you in your career?
- LW:** It certainly got me through my Ph.D. with flying colours. But of course, as I say, like women of my generation, I married and had to follow the boss around which is how I finished up, with Thimann, which was really, you know, not in that particular area. So although it was a great experience and one recalls it with great pleasure and it probably taught one, you know, things about that particular approach to research and so on, it could not be immediately applied to what I was then doing subsequently. But it is something one looks back on with great pleasure and gratefulness, if that's the word: gratitude is the word.
- VM:** One thing I wasn't clear about and must have slipped by: did you go to Berkeley in the middle of your Ph.D. and then go back to Copenhagen and finish it?
- LW:** Yes. I think I went after two years and then went back for a year afterwards, something like that.

VM: And it was in plant physiology so therefore highly relevant to what you...

LW: And photosynthesis in particular, yes.

VM: So some me of what you did in Berkeley was built into your thesis?

LW: Yes, but, you know, there were other things as well. But mostly to do with chlorophyll and photosynthesis.

VM: Well, thank you very much indeed for coming and telling us and helping us to relive.

LW: It has been a great pleasure for me too, thank you.

After a break:

VM: New things have arisen. You have memories of social occasions with the Calvins.

LW: Yes, very much social occasions. Both once in Stockholm, I remember, I think there was a big congress and a lot of us (*Editor: Nobel Prize winners?*) were there. Melvin had taken us all out to dinner in a restaurant. As happens in Scandinavia in general, in Denmark as well, service is very slow in restaurants and this did not go down well with Melvin and he was almost on the point of walking out because they wouldn't serve him quickly enough. There was rather an unpleasant episode, but it was very funny on one level.

They tended to, you know, when they travelled throw their weight a bit about because Gen also...we had an episode in Copenhagen once when one of the children had swallowed a marble and everybody had to rush off to hospital; the poor child had to be pumped out although everybody assured her that it would doubtless come out in a natural way. The poor child was pumped out in the end. And I also remember Melvin actually once, at one of these big rave-ups if I can put it that way, in Switzerland (*Editor: Lindau meetings?*) where one of the Bernadottes arranged these Nobel Laureate meetings in the summer, where they had students and they had all the professors there. And Melvin was there, and Gen. Melvin had a great complaint and that was: we were, of course, all invited and put up in a nice hotel with a swimming pool and all the rest of it, but they made him pay for ringing up Berkeley to confer with his son to find out what he was doing. This he found was not good enough and he was not very impressed. There were all sorts of little episodes like that, shall we say. But on the whole, he was always very supportive to his friends and collaborators and so on.

VM: After he got the Nobel Prize in '61, he was gradually drawn into the Washington scene. He was appointed to Kennedy's (*President's Science Advisory*) Committee and so on. You would have seen him through the years, on and off, would you?

LW: On and off, yes.

VM: Did you notice him changing from being a 100% scientist to being, perhaps, only a 98% scientist?

LW: Well yes but, you know, no less than 98 I would think. But then he got into these sort of, shall we say, slightly fringe projects like growing *Euphorbia* in the desert and

trying to make oil and this kind of thing which perhaps had moved a bit away from the sort of cut-throat science that he had done before.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name LISE SCHOU WILKINSON
Date of birth 6 May 1924 Birthplace Copenhagen
Father's full name SVEND AAGE SCHOU
Occupation Professor Birthplace Copenhagen
Mother's full name ELLEN SCHOU née FJERDINGSTAD
Occupation housewife Birthplace Copenhagen
Your spouse GEOFFREY WILKINSON
Occupation Professor Birthplace TODMORDEN, YORKSHIRE
Your children ANNE & PERVILLA

Where did you grow up? Copenhagen
Present community LONDON
Education Copenhagen University & School of Pharmacy

Occupation(s) Medical Historian

Areas of expertise History of infectious diseases

Other interests or activities _____

Organizations in which you are active _____

Chapter 55

SIR EDWARD (TED) P. ABRAHAM

Boars Hill, Oxford

August 25th, 1997

VM = Vivian Moses; EA = Edward Abraham; SM = Sheila Moses

VM: This is a conversation with Edward Abraham in Oxford on August 25th, 1997.

Can I start by asking you how you came to go to Calvin's lab.?

EA: I had a Rockefeller Foundation fellowship and the work that Calvin was getting involved in was of general interest. Also, I think I was interested in trying to make some use of isotopes. Those two things together were really responsible for me going to Berkeley.

VM: What had been your original scientific education?

EA: My original scientific education was, I suppose, organic chemistry. I read chemistry here...

VM: In Oxford?

EA: In Oxford and at the end of the war I had become rather more of a biochemist than a simple organic chemist. The work that Calvin was doing was, in fact, very interesting.

VM: What was your position at the time when you went? You were still in Oxford, were you?

EA: At the beginning of the year of the war opening, I was in Stockholm, again with the Rockefeller. I was in Stockholm when war broke out. The department there was run by Von Euler who wanted me to...and it was a curious department in a way, it was a mixture of Swedes, of Jewish refugees from Germany and people...well, even one or two confirmed Nazis, some of them quite nice people but they were confirmed Nazis! So it was a curious...And Euler wanted me to stay because at that time in Sweden it was generally thought that the Germans were going to win the war and that Sweden might keep out of it, that England would lose the war. So he thought this would be attractive to me. But I said "no, my duty, if you call it that, was to return to England".

VM: What level of development were you at the time? Were you a graduate student in Sweden?

EA: I had just got a D.Phil.

VM: So, in effect, you were a postdoc.

EA: Oh yes, I was a postdoc. Von Euler had a place in the laboratory. He had the top floor more or less which was his house. At the one end there was a picture of King Gustav but at least one person who didn't like Von Euler very much said if you turn the picture over there was one of Hitler on the other side; which one showed depended on who came to dinner!

VM: Did you ever turn it over?

EA: No. Well, I was no position to do that. I don't know whether that's true: it may or may not be true.

VM: So, anyway: you came back.

EA: Anyway I came back across the Atlantic eventually from Bergen. It wasn't very easy to get back. I had a...all this is irrelevant, really. I had a...before I left Stockholm for Bergen, to try to get back, I got an infection in a swimming bath and I went to England and I seem to have an infection in the foot". He looked at it and said "well, I can tell you that unless you take these tablets the only way you will get back to England is in a wooden box".

So I did take them and they were...I didn't know at the time...they were one of the earliest sulphonamides, not penicillin, that wasn't known at that time as something that was useful. So I got back and went back to Robinson...

VM: He'd been the person you'd been working with before? Robert Robinson?

EA: Well yes, he was I suppose. He was the Wainfleet Professor of Organic Chemistry, was one of the great organic chemists of the day, I suppose. When I got back people were thinking of what kind of experiments could be looked into which might be useful for the war effort. Robinson said "I think if you go and see Florey (*Sir Howard Florey*) and he would be interested in your background, and he would be, that this might be something that he would get you into. That's how I came back to Florey and started work on...forget what it was...£300 pounds a year or something like that.

VM: In the penicillin project?

EA: Yes, after a short time. We were looking into...Florey was interested in some aspects of shock and wound shock, and so on but it looked as though nothing was going to come to that. The department was very poor (*at the time*).

VM: That was Pathology, was it?

EA: In Pathology.

VM: He was Professor of Pathology as I remember.

EA: Florey was. He had fairly become Professor of Pathology from Sheffield. He was an Australian by birth and he wanted to do chemistry, actually...(but) the Australians told his father there weren't any jobs for chemists, worthwhile chemists, in Australia then; he'd better do medicine. I remember Florey saying to his...in College...Bertie was my

son...tells me that he wants to do...I think it was physics. But I said to him "you had better do medicine. Then you won't starve". He did medicine.

VM: What was your position in the department? Did you have a staff post?

EA: No, I didn't, no; not at that period of my life. There weren't any staff; well, the School of Pathology there was Florey about and Chain (*Ernst Boris Chain*) who had recently come. Neither of them had staff. Florey had...was virtually the only person with a staff post..

VM: On what basis were you employed?

EA: I think I was paid by the Medical Research Council.

VM: Was that still the case by the end of the war when you decided to go to Calvin?

EA: No, things were about to change then in that at that time the work on penicillin at Oxford, in the School of Pathology in fact, had demonstrated that penicillin was an important substance and it became very important during the war because it looked as though, if enough of it could be made, it would be of great importance in curing large numbers of troops. It became an American-Anglo-British project, too. There still exist the papers that were exchanged between American...there was a large number toward the end, there were some hundreds of American and British companies trying to produce large amounts of penicillin.

VM: At the end of the war you went to Calvin in which year was that?

EA: In 1948.

VM: What was your position then, what was your position in the department?

EA: In the department in 1948? I think I had a better...what looked as though a more likely or a secure position. But I wasn't — let me think. When I became...I think that's... no I don't think it's in my *Who's Who* or anything...It was all rather vague until, well about the time I went to Berkeley, I suppose. It looked as though there would be a more permanent position. It was first of all what was called a "research officer" and then it was a reader and professor and so...

VM: You decided to go to Calvin. Presumably you had heard of the work he was doing, read some of the papers, and decided it was of interest to you.

EA: Yes.

VM: And you got a Rockefeller?

EA: Yes. Calvin...well, I suppose Florey got it for me. But Calvin was using isotopes, radioactive isotopes, and this was something of general application. (We stayed...no that was later, a later visit, we stayed with the Calvin. They had a flat there in Berkeley.) And then Calvin came here as a visiting (*Eastman*) professor (*in 1967-68*). I remember he came up here (*to Abraham's house*). My memory is that he was rather cold; it was in winter. I took him to dinner in College. He was partly...Calvin was partly responsible for...I mean, later on, when we started finding cephalosporin compounds and so on, he...Calvin came here as a visiting professor and I said "with some royalties we'd got we were hoping to set up, among other things, a visiting

professorship here; have you any views on this?" And he said, "I think, there shouldn't be so many restrictions as applied to the visiting professorship I had". And this one is still going but I'm not sure how...So we or I took some notice of (*this*) and I think the visiting professorship that I set up, which didn't require the visiting professor to give thirty lectures or this kind of thing, and it was much more relaxed and it could be....hopefully the visiting professors could have general conversations with people and do what they really wanted to do. I think the way this was structured was to some extent at any rate, due to what Calvin said to me when I spoke to him about the project.

VM: But to take you back to 1948, which is the period that we are directly interested in, had you met Calvin before you went to Berkeley?

EA: No, I don't think so.

VM: How did you travel? By sea did you go?

EA: I went by sea, I'm sure, on one of the *Queens*, I think it was. Then the Rockefeller people in New York said "did you want to fly to Berkeley or do you want to go by train?" I said I would rather go by train because I would see much more that way. So I went by train to Berkeley.

VM: Were you by yourself?

EA: Yes, I was by myself.

VM: When you got to the other end, what happened? Somebody meet you or were you dumped, had to find your own way?

EA: No I think I stayed at a place called International House in Berkeley, which you probably know. How it was that I...or who it was who said this is a good place to stay in, the food was reasonable and it's not expensive, and so on. I don't know. I spoke to some people from Berkeley on the train; maybe they...I don't know. But International House — they still write to me.

VM: Well, they're still there.

EA: They must be: I mean, they ask for money so they must be! They are not within our scope, actually, but I'm sure...

VM: You went to the lab. and you met everybody, you met Calvin and you met the others probably?

EA: There was a man called Heidelberger who was there.

VM: Charlie Heidelberger.

EA: Charlie Heidelberger, yes.

VM: Who else do you remember as being there at the time?

EA: I think Charlie Heidelberger particularly was the person I did some experiments with. The others, if their names were read off, I'd probably remember them clearly.

VM: Let me ask you the next question. Do you remember which building you worked in?

EA: The Donner.

VM: There were two buildings there you may remember. There was the Old Radiation Lab., which was a wooden building, where the photosynthesis work was going on and that work was led by Andy Benson.

EA: Oh yes, well I remember him.

VM: ...and Al Bassham — do you remember Bassham?

EA: Yes.

VM: In the other lab., I think there was Bert Tolbert and Dick Lemmon.

EA: Tolbert, I think, was some sort of organiser in the department. I didn't have any...There were still government restrictions in 1948. For example, I couldn't be in the Donner at night.

VM: Oh really?

EA: They had armed guards on the place. Eventually, from the Atomic Energy Commission, I think it must have been...or through them, I got permission. That was, in fact, the day I was leaving — probably an accident but that's how it happened.

VM: So you worked with Charlie Heidelberger. What did you do?

EA: We did some radioactive work with radioactive biosynthetic work which was of interest to me.

VM: Chemical work?

EA: Yes, chemical work.

VM: Presumably you published some of that at the time.

EA: It was a little paper published on that. There were other things...I'm trying to remember what they.... There was some catalytic reactions which were potentially dangerous in the sense that they might go wrong and the place would blow up. So there were special precautions with relation to this. Calvin came in; as you say, he was not domiciled, shall we say, in the Donner but he came in quite frequently and people consulted him quite frequently; I think that's how it was.

VM: So you were one of those; you consulted him frequently?

EA: Yes. He was the sort of boss, if you like, of that show.

VM: You say he was not the boss?

EA: No, he *was* the boss of that particular...But where Geoffrey Wilkinson was was on The Hill and he...well, his work was still more or less secret I think with Seaborg.

VM: Was Lise (*Schou*) Wilkinson in Calvin's lab. at the same time as you?

EA: I don't remember meeting. My first memory of Lise was in England here and that was partly...well, I don't know partly but I met Geoffrey Wilkinson the first time at The Royal Society after he came back and Lise was, as you know, was a Dane and my wife was Norwegian so they had a contact, if you like.

VM: So you didn't know Geoffrey in Berkeley?

EA: Oh yes.

VM: Oh you did?

EA: Yes, I did. In fact he took me on very interesting car drives: to Yosemite, for example, we walked up to the top of the (*immediate?*) Yosemite and slept, after slinging our rucksacks high enough to avoid the bears. Then he drove me on the other side up to a lake, which was quite high and we slept out. I always think of that as perhaps the coldest night I have ever spent. And the he drove me...he did a number of other...took me on other trips and so on. His work was still secret as far as I was concerned. He went over in, what was it?, '43 I think it was.

VM: I seem to remember that you were one of the early postdocs. in Calvin's lab. Can you remember: were there other people, other visitors there at the same time as you?

EA: Visitors? There...no in Calvin's lab.; I go back to...Well, Calvin asked me to stay, actually, and I said "well, but that wasn't in..." I had decided I was not going to stay, anyway, and I said that I think...oh yes one of the preconditions that the Rockefeller Foundation made was that I should not stay. I told this to Calvin and there's an easy way out: he said "oh, that's no problem and we could easily get around that".

VM: When you were there, as far as you remember, were most of the people Americans? Because later on it certainly became a very international group. Was it like that when you were there?

EA: They were almost all Americans. This was, what? 1948.

VM: Maybe the international character developed rather later than that.

EA: I mean later we went to Berkeley on visits several times. I went to the Round House which wasn't in being when I was there.

VM: What did you think of that as a building?

EA: Well, it was interesting. I thought there were different opinions from different people as to whether this was the best way of organising people. Some people thought it was good and some people didn't think it was.

VM: How did it strike you? After all, presumably here in Oxford you have always worked in conventional buildings, square, oblong buildings with rooms off corridors. That was a very different, open plan type of thing. What sort of impression did it make when you saw it? On you?

EA: I was uncertain whether it would have appeared to me, if I had worked in it, to be the idea. I simply didn't know without work. I mean, the idea was that people should

have...there should be contacts between people which were valuable and that this was a way of getting them together. I don't know. It's still very much in being, isn't it?

VM: But it's has changed its character.

EA: It's has changed its character.

VM: A great deal.

EA: That wouldn't surprise me.

VM: Well, because the group that occupy...the people that occupy are no longer the unitary group that they used to be. It has inevitably become fragmented and bits of separation in the building. For that it's not a good building because it is intended as an open unitary building and once you try to divide it, it's not successful, I think. Much less so than it used to be. However, in your time in Berkeley you presumably visited at least sometimes the old wooden building, the Old Radiation Lab. Do you remember that building?

EA: That was the...the Donner, the Radiation Lab.; yes, I do remember that. It wasn't very far from the Donner.

VM: That's right; but that's where Calvin had his photosynthesis work going on.

EA: Yes, that's right.

VM: And I don't know whether you remember but that was pretty much open plan. And I think it stemmed from that idea, from that environment that the concept of the round building grew.

So how long did you stay in Berkeley on that trip?

EA: Not long; I mean for other reasons I wanted to go back; I suppose six months, or less.

VM: Working all the time with Charlie Heidelberger?

EA: Yes.

VM: When you left Berkeley, did you come directly back to Oxford?

EA: Yes, I did. That was understood that I should come back. By that time, it was getting... a certain number of people in the Pathology Department were looking forward almost certainly to permanent jobs. I think Chain probably...but then Chain left, he and Florey did not get on exactly

VM: That's when Chain went to Imperial? he went to Rome...

EA: He went to Rome and Chain, the last time I met...Oh yes, and he had a fine establishment in Rome with flats and so on. I remember, I think, the last time talking to him in Rome he said he'd been asked to return to England, he'd like to return to England, but he said "can you tell me how one can live in England without paying any income tax!" And so I said "well, actually, I'd like to know, but I have not discovered no way myself by which that can be done".

- VM: You spent the whole of the rest of your career in Oxford, did you, in the Pathology Department?
- EA: Well, except for visits to places like Australia and the Middle East, and so on. We saw a good deal of the world in one way or another.
- VM: But this has always been your base?
- EA: Yes.
- VM: You have now retired, have you?
- EA: Very much so because one retires at the age of, what is it? 65 here at that time. I am 84!
- VM: Oh really? That was nineteen or twenty years ago that you retired.
- EA: That's right, that I retired. But I haven't retired — I've kept a room until very recently in the School of Pathology and I had a secretary; that's how it is. I thought when we bought this house, which was about thirty years ago, that if I had nothing to do when I retired at least, if I was fit enough I could do something in the garden, because it has a wood as well as a garden attached to it. But it didn't turn out quite like that.
- VM: Were you not fit enough or you didn't have the time?
- EA: Well, I had other things to do which were concerned with the department and other things. It wasn't that I was looking for things to do in the way I thought I might be.
- VM: You progressed up in the usual way through the academic scale and became a Professor of Pathology, were you? What was your title, formally?
- EA: Professor of Chemical Pathology.
- VM: Have you maintained any contact with the Berkeley people over the years? You say that Calvin's been here.
- EA: Calvin's dead now, of course. The last time we saw him in Stockholm, I think. Heidelberger is dead. The other people...no. I don't think; oh...Mary, the secretary: what was her name? Mary, Mary...
- VM: Marilyn.
- EA: Marilyn.
- VM: Marilyn Taylor.
- EA: Marilyn Mack...was it Marilyn...?
- VM: Taylor.
- EA: She might have got married. I wonder if she got married during my...? Anyway, I... She occasionally wrote to me.

VM: She is collaborating with us on this project. She's still here and she's helping with some of the transcriptions and some of the finding people for us. She's still very much part of the scene.

EA: She can't be young any longer?

VM: None of us is young any more! She's no older than the rest of us.

EA: Oh, I'm not surprised. But I think the last letter from Berkeley, certainly the last one I replied to, was from Marilyn. That's perhaps true of a number of people who went through Berkeley.

VM: When was the last time you were there and you say you saw Calvin was in Stockholm.

EA: I saw Calvin in Stockholm because we went to Sweden at a time when there was a Nobel Prize giving. He went to that as an old...and I got invited because we were there so I met him there. He seemed to be all right. That was after we stayed one time when we were in Berkeley. They asked us to stay; they had a flat below and...what's her name?

SM: Genevieve?

VM: His wife?

SM: She was from Norway, too.

EA: I see; well, she was very nice to us and she organised, you know: when we wanted to go to the airport, well she turned on a car and it came. We had meals with them and so on. Perhaps at that time; it was a long time ago...I don't think Calvin was seriously ill at that time. I don't think he was going around in a wheel chair; he had an electric wheel chair at one time, didn't he?

VM: he had an electric wheel chair but that was not, as far as I remember, because he was ill but because the lab. had become separated and they'd had to move down to the Life Sciences Building after they pulled down the wooden building and before the round one was built. He had a wheel chair (this electric trolley) to travel from his office in the Chemistry Building down the hill and back again. He had always, or at least for many years, had a weak heart and I think it was for that. I don't think he was specifically ill. Once the lab. moved back together, then I think he stopped using it. And, of course, when he died recently, although he had been failing in recent years, he was 86 years old, nearly 86 when he died. So he was not a healthy man but he was not seriously ill for much of his life.

When you say you saw him in Stockholm at a Nobel Prize celebration, was this his own Nobel...?

EA: No, it wasn't.

VM: It was later than that?

EA: It was later than that. It was...somebody from this country. Anyway, he seemed in, you know, fairly good shape.

- VM: Oh, I think he was apart from illnesses, odd illnesses here and there, until fairly recently he was in quite good shape. I thin that particularly after his wife died, he was very upset with that for some time but that was ten years ago now.
- EA: She looked after a welfare club, or “Newcomers Club” it was called in Berkeley and we were...as we became interested in a newcomers club here I think partly in connection with what we’d seen in Berkeley.
- VM: Well, the tape’s almost coming to an end. So I think I’d like to thank you very much for digging down into your memory. Obviously, some of it is a long way away now. It’s interesting to hear what you have said and we’ll see how it will build into the story that we are going to write. Thank you very much.
- EA: All right.

The respondent failed to return this form which has therefore been
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Ther

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University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name SIR EDWARD PENLEY ABRAHAM

Date of birth JUNE 10, 1913 Birthplace _____

Father's full name ALBERT PENLEY ABRAHAM

Occupation BE Birthplace _____

Mother's full name MARY HEARN

Occupation _____ Birthplace _____

Your spouse ASBJORG HARUNG

Occupation _____ Birthplace NORWAY

Your children 1 SON

Where did you grow up? AT SCHOOL IN SOUTHAMPTON

Present community BOARDS HILL OXFORD

Education QUEEN'S COLLEGE OXFORD

Occupation(s) PROFESSOR EMERITUS OF CHEMICAL PATHOLOGY

Areas of expertise CEPHALOSPORINS AND RELATED ANTIBIOTICS

Other interests or activities WALKING, SKI-ING

Organizations in which you are active ROYAL SOCIETY

Chapter 56

HELMUT METZNER (with Barbara Metzner)

Tübingen

September 13th, 1997

VM = Vivian Moses; HM = Helmut Metzner; BM = Barbara Metzner

VM: This is a conversation with Helmut and Barbara Metzner in Tübingen on the 13th of September, 1997.

Helmut, can I start by asking you about your early education, where you had it, which subjects, and what led you to go to Calvin's lab. in Berkeley?

HM: I was a student in the University of Göttingen and a specialty in plant physiology and plant biochemistry was at that time photoperiodism. So, we studied the reaction of plants to various light qualities, light intensities, but we never worked on photosynthesis and at that time we had not the opportunity to work, for example, with radioactive materials. Then, one day the institute had a visitor from the Rockefeller Foundation and we had a long conversation. Then he said "wouldn't you be interested to come to the United States?" He promised to go to Berkeley.

VM: What was your position at the time?

HM: My position at that time was a Lecturer for Plant Physiology. Then after a while I received this invitation from the Foundation and we decided to come to Berkeley. This field of photosynthesis was new for me, some techniques were known like the chromatographic separation, but then we learned how to apply radioactively marked compounds to identify them and, what was even more valuable for me, was the discussion with many scientists which at that time were in Calvin's lab. So, we learned so much of photosynthesis that after my return to Germany I decided to continue this line of research so we left more or less the old photobiology and concentrated on photosynthesis research.

VM: OK. You are going very quickly. I'll go through these topics rather in more detail! When you decided to go to Calvin in Berkeley, did you already know about his work?

HM: Yes, I knew about his work. I had seen several of these publications and I was very much interested to learn more about this type of research and the methods which could be applied at that time in Berkeley.

VM: Had you met Calvin himself before you went to Berkeley?

- HM: Yes, I met him in Göttingen, but that was after my contact with the Rockefeller Foundation. And then we could talk about the research which at that time was just going on in Berkeley. Then he invited me to come to his place.
- VM: That must have been, when? In '55, '56?
- HM: It actually was '55.
- VM: He was on a big trip through Europe.
- HM: He was on a big trip and so he just interrupted this round trip in Göttingen.
- VM: Did he give a lecture here?
- HM: No, he didn't give a lecture: we were just discussing in a small circle.
- VM: Then you travelled to the US...when was it?
- HM: It was one year later, it was '56.
- VM: By sea did you travel there?
- HM: Yes, that's right.
- VM: How did you travel across the country, across the US?
- HM: We had no car so (*indecipherable*) we made all our round trips with Greyhound.
- VM: That was the first time you had been in the US?
- HM: It was our first visit to the United States, that's right.
- VM: Incidentally, what was your first impression of America when you saw it?
- HM: We had read so much about these places. For us these big cities were very interesting, the universities were interesting. But we liked to travel around to see the landscape, places like Arizona and so on were quite new for us. And so we used the free time to travel around by bus.
- VM: When you arrived in Berkeley, can you remember what it was like when you first arrived? Can you remember arriving? What happened? Did somebody meet you? How did you actually arrive on the West Coast?
- HM: We arrived with a little freighter coming directly to the West Coast and one of the passengers which was on board took us with him to Oakland and from there we went to Berkeley. That was our first impression from the United States.
- VM: When you got to the lab., you went presumably direct to the lab. in Berkeley? How did you find somewhere to live?
- HM: We found pretty fast a little place, not too far away from the campus, and we stayed there so it was not necessary to have a car. We spent most of our time, I must say, in the laboratory and even sometimes during the night I had to go to change chromatographs and things like that.

VM: I think everybody who was there at that time spent many nights changing chromatograms. How did you decide what you were going to do? What was it? How did you come to this decision? Who did you work with the lab.?

HM: After the first discussion with Calvin, I told him what we had already done in the field of photobiology and that I would be very much interested in these reactions of the Calvin cycle and to learn how we can identify such compounds, and so on. And so we were then advised to go to this lab. and started our work with various kinds of organisms which we knew partly already from our work in Göttingen.

VM: But many of the techniques in the lab. were not familiar to you, they were new for you.

HM: Many techniques were completely new, that is right.

VM: Who showed you? How did you learn?

HM: We learned by discussion with the guests which were there and with some of the scientists which belonged to this lab. so this was not too difficult for us to get acquainted with these new techniques. After a while we could make our own programme which was close connected to the experiments which were going on at that time.

VM: As I remember, Barbara and you worked together in this.

HM: We worked all the time together, that's right.

VM: What was the problem you began to work on?

HM: We worked on the first fixation products, that was interesting for us. We wanted to know how fast the CO_2 is incorporated, which are the compounds which we can identify first. Whereas the further steps at that time were already well known by other experiments in the group.

VM: What did you do to take this investigation forward?

HM: We made these investigations with the thin-layer chromatography and with paper chromatography at that time but used very short light times to get the first fixation products.

VM: How short is very short?

HM: It was sometimes less than one minute.

VM: Less than a minute!

HM: Sometimes seconds.

VM: This sort of short-term photosynthesis radioactive study, some of this work had been done already by Benson and Bassham in the early years when they discovered PGA.

HM: These experiments which we performed were really the first ones with very short light periods. Of course, PGA was at that time known, but it was not very well known

what happened after these very first steps and whether PGA is really the first product or whether it has a precursor.

VM: What did you find?

HM: We found that there is a strong influence of the CO_2 but these were experiments which we could not perform in Berkeley any more. But we tried to do these experiments when we returned to Germany.

VM: You said there was a strong influence of CO_2 .

HM: There was a strong influence of CO_2 and we had the impression that the pressure of the CO_2 had a strong influence on the incorporation.

VM: I don't remember that work. Can you remind me what you found? You did that when you came back to Germany?

HM: When we came back to Germany we did these experiments with mass spectrometry to see the influence of the CO_2 , how fast is CO_2 incorporated, how is the isotope discrimination in these experiments, what do we see with the oxygen which comes out. Here we could repeat these experiments with stable oxygen isotopes.

VM: When you came back here, did you build for you the same sort of facilities that had existed in Berkeley? The chromatography and the radioactive stuff, and so on.?

HM: Practically, yes. We started with these experiments and built it up here because we could use these types of experiments in the courses with the students and later on we could make the other experiments with mass spectrometry in other departments of the university. We never had our own instruments.

VM: If we can come back to Berkeley. I'd like to talk a bit later about what you did in Germany when you came back here. Did you spend all your time in Berkeley working on this problem of short-time photosynthesis experiments?

HM: Yes: it was practically the problem which we worked on all the time. We tried to find the time to discuss with as many colleagues and look around to other groups which at that time were working in Berkeley so that we learned as much as possible from the problem of photosynthesis.

VM: I'm not sure that I remember this correctly, but I have a feeling that you used phosphorus as well as C^{14} , radioactive phosphorus as well as C^{14} .

HM: No, we have never worked with radioactive phosphorus. We worked just with C^{14} .

VM: OK; I must have got that bit wrong then. Who were your collaborators, talking collaborators, discussing collaborators in the lab. in Berkeley?

HM: The discussing colleagues were mostly Helmut Simon and Otto Kandler. They were at the same time in the laboratory and they were working in a very related field of experiments so we that had a close contact just to these. We had, of course, discussion with the other guests of the laboratory like Chris Van Sumere, Bob Rabin, so that we learned what their special problems are, how they attacked these problems and they knew other literature citations than we knew. So I must say that during this year I

have learned very much in the whole field of the influence of photoreactions on plants.

VM: When you first came to the lab., was the atmosphere generally relaxed — what I remember as a relaxed atmosphere. Was that the same sort of atmosphere that you had already experienced here before you went to America or with this sort of thing new for you?

HM: It was new for us because this group was much bigger and more experienced, I must say, than it was at that time in Göttingen. We were a smaller group which was concentrated on related fields of photobiology and we were at that time restricted with our techniques. At that time we could not buy radioactive materials here and we had not very complicated instruments. We had to work with manometry and other techniques which we could forget about during our stay in Berkeley. That does not mean that we did not apply them afterwards again because at the university we had to teach students and we had not time to spend all our effort to research. That was quite a difference and quite an experience for us in Berkeley. So we could use the time much more economically than we had in the university.

VM: Of course, before you went to America you were already teaching in Göttingen.

HM: Yes, I was teaching plant physiology. I had studied botany and physics, especially, I must say, physical chemistry, and so I always tried to work in a field in which I could apply the experience of physical chemistry and of plant biochemistry.

VM: What sort of relationship did you find you had with Calvin himself? Did you see him a lot? Did you discuss technical details with him a lot?

HM: I saw him a lot. We could discuss all these problems with him. He was interested in these other photobiological questions too so that we had a great profit from this stay in his laboratory.

VM: Did you find him a personally stimulating person? I know he had many ideas but many of his ideas were not that good. Some were good and some were not good. How did you find him?

HM: I find him very encouraging. I must say that because in the university at that time the personal contact to the heads of the laboratories was much poorer.

VM: In Germany?

HM: In Germany. So everybody had a small group of its own. But here we had guests from all over the world and that was very encouraging. And Calvin himself was interested to hear what had happened in related fields.

UP TO HERE

VM: What do you remember of the social activities in which you were involved in Berkeley? You said you travelled. Presumably there was social mixing with people in the lab., maybe people outside the lab. How did you...did you spend all your time in the lab., or was there some of your life outside as well?

HM: I liked the climate very much and, since we had no car, somebody from the laboratory came over the weekend and picked us up. And so we made tours together, looking

around, so that in California I could visit several interesting places which we would not have seen alone.

VM: Many of us remember evening activities with mixing in people's homes: did you involve yourself with that as well or were you always working at night?

HM: We were not always working at night. But we had not the facilities to invite parties to our rooms but we met outside the laboratory, too. I cannot say that we had spent all the time just in the laboratory. We really used this first experience in the United States — afterwards we had been there several times but this was really the first impression — how a good group can cooperate and what (*indecipherable*) discussion can bring.

VM: You remember, of course, the seminars there and presumably you also contributed to the seminars. Were these sorts of seminars new to you? Were these the sorts of seminars that you had previously experienced in Germany?

HM: No. The form of the seminars was quite new for us. In our universities in Germany seminars were long prepared or there were invited guests which were just travelling around and were invited to talk about their activities. Seminars like those which we had in Berkeley were, at that time, unknown to us.

VM: How long did you stay there in that lab.?

HM: We stayed a full year.

VM: Then you came back to Germany to the same position in Göttingen?

HM: First to the same position in Göttingen and I could not find my old group because part of the students had already finished in the meantime. But we tried to copy a working group as we had seen this in Berkeley.

VM: Difficult?

HM: No, that was not difficult.

VM: How long did you stay in Göttingen before you left?

HM: I stayed until '61. In '61 I came here to Tübingen to build up a new group.

VM: You came with a position to Tübingen where you could really build your own research activities?

HM: That is right, yes.

VM: You have been here ever since, which is now 36 years?

HM: That is right. In that time we are here. Here in Tübingen was very active group working in the same field as the group in Göttingen. They were interested all in photobiology but in these aspects like photoperiodism and photosynthesis was new here. So I had to introduce the techniques and in seminars and in lectures all the special things which we had learned and we tried to develop this here even further.

VM: When you first came here in '61, you came as professor?

HM: I came as professor, yes.

VM: And Director of the Institute?

HM: Yes, Director of the Institute of Plant Biochemistry.

VM: So you really were in a good position to develop in the ways you wanted to go in at that time?

HM: That is right. We had pretty old rooms at that time, and for the students it was a new field of science, so it took a little bit of time to organise such a group. But after a couple of years I had a very active group here.

VM: What was the funding position like in Germany at that time? Was it reasonably easy to get research money?

HM: We had at that time the Deutsche Forschungsgemeinschaft and we could apply for research grants there and this was the main source. What we received here from the local government was just to cover the costs for courses but not for research. For all these special problems, we had to apply for money from the Forschungsgemeinschaft.

VM: It was reasonably successful. You could expect to get funds?

HM: At that time it was rather easy: if you had a good project, you received the money.

VM: How big did your own research group become here in Tübingen?

HM: With the active students which were performing the experiments for their thesis, we were at least together about twenty persons.

VM: That's a significant body of people.

HM: Yes.

VM: What sort of contact did you maintain with the Berkeley group? Did you go back there on occasion?

HM: No, we could not go back except for a short visit. But we exchanged our reprints. We wrote letters and we kept contact with those guests which were there at the same time as we studied in Berkeley. We exchanged our experiences and we visited each other, for example with Chris Van Sumere in Belgium — that was a contact which we tried to keep.

VM: Looking back all those years ago, what do you think were the most important contributions that the Berkeley experience made to your own development?

HM: If I look back, I think for my own development it was to learn these close contacts, I may say the permanent discussion in the group. When the results of Calvin's experiments which, at that time, were very exciting, so everybody tried to use the techniques which he learned and to keep the ideas which he picked up during his stay in the United States and transferred these to his own group. So we worked in the field of photosynthesis with quite different techniques sometimes, but always looking to the path of carbon, if I may say so.

- VM: Did you find that your own personal way of working changed as a result of being there? The relations that you might have developed with your colleagues, with your students, were you influenced by what you had seen in Berkeley? Was that a factor, do you think, in what came later?
- HM: I think that was not the main influence. You always try to keep good connections to colleagues, not only in the own department but with related departments in chemistry, in physics. It was the technique and it was the richness in ideas, I must say, which impressed me.
- VM: Many people have talked about the influence of that particular building in Berkeley, the Old Radiation Lab., on the way the group actually operated. Do you feel that had any reality?
- HM: I must say that the building which we found here was not better than the Old Radiation Lab. in Berkeley. What I observed here during the development of the new university campus, I must say that it is close connection in a group which is much more effective than living in a very modern building where everybody has his own rooms, separated from his colleagues.
- VM: So you liked the idea of ORL?
- HM: I liked the idea of such a group, yes.
- VM: You visited the round building at some stage later on?
- HM: Yes, I have seen the round building later on.
- VM: How did that strike you as a building?
- HM: Well, I was not very impressed. When I remember this Old Radiation Lab., I compared this round building with the buildings which we have here in the campus and which we have in many other German universities. This typical atmosphere of the Old Radiation Lab. was not transferred to this new building.
- VM: It is, of course, always difficult to make a change between the old and the new and retain the flavour.
- HM: Yes.
- VM: When were you last in Berkeley and made contact with these people?
- HM: I suppose it must be about '85' we came back from a round trip where we visited Mexico and so I could not spend very much time. It was just the impression how it looks now on this campus.
- VM: Barbara, you haven't actually said anything in English all this time. Won't you like to say just something?
- BM: Yes, I would like only to say that it was a big family in Berkeley and that is what I thought the best thing I have ever seen when I was working together with Helmut in another lab. or in our own lab.

VM: Well, I think many of us who were in Berkeley at that time feel like that. I am very grateful that you spent the time and that you have tried a little bit again to relive those times forty years ago. Thank you very much.

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Helmut Metzner

Date of birth 15. 9. 1925 Birthplace Osnabrück

Father's full name Friedrich Metzner

Occupation Teacher Birthplace Celle

Mother's full name Anna Metzner, born Dietrich

Occupation - Birthplace Osnabrück

Your spouse Barbara Metzner, born Reinboldt

Occupation - Birthplace Bad Oeynhausen

Your children _____

Where did you grow up? Osnabrück

Present community Tübingen

Education Humanistic Gymnasium, Osnabrück

University of Göttingen

Occupation(s) University Professor

President of the European Acad. for Environm. Affairs

Areas of expertise Plant Physiology

Biochemistry

Other interests or activities Ecology

Organizations in which you are active _____

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